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PROBLEMS OF THE GLACIALIST[1]

By FRANK LEVERETT

A YEAR ago, at the New York meeting of this association, I discussed the Pleistocene glaciation of the northern hemisphere, as a contribution to the symposium on the centenary of glacial geology. This year I will take up some of the problems confronting the glacialist. Last year's paper outlined the results of past work. The present paper endeavors to outline future work. Some of the problems are matters of local application, but the majority of those here considered are of world-wide bearing.

THE PROBLEM OF GLACIAL EPOCHS

Inasmuch as the normal climate of the earth has been non-glacial the cause for glacial epochs has excited wide interest, and its solution has been attempted by a wide range of students, including astronomers, physicists and biologists, as well as geologists and climatologists. Unfortunately, the climatologists, who should be the leaders in this study, have been baffled by the complexities of climatic factors. They differ widely in the interpretation of present climate, as well as in that of past climates. Some of the leading ones have opposed their speculative views to the plain teaching of observations on the existing ice-sheets, as has been clearly shown by Hobbs in his recent book on the glacial anticyclones.[2] The doctrine of circumpolar cyclones, to which Ferrel Maury and James Thomson contributed in the 1850's, was still supported by Hann in 1897, when he wrote in his "Klimatologie" (p. 543):

> The whole Antarctic circumpolar area presents us, as already stated, with a vast cyclone, of which the center

[1] Address of the retiring vice-president and chairman of Section E—Geology and Geography, American Association for the Advancement of Science, Des Moines, Iowa, December 30, 1929.

[2] "The Glacial Anticyclones: The Poles of the Atmospheric Circulation," Univ. of Michigan Studies, Scientific Series, Vol. IV, 1926, by William Herbert Hobbs.

is at the pole, while the westerly winds circulate around it.

After observations on the Antarctic continent by members of Scott's expedition had clearly indicated that an anticyclone, and not a cyclone, lies over that continent, Hann, in correspondence with the British Meteorological Office, granted the presence of an anticyclone on the immediate surface of the continent, but maintained that at a moderate elevation the cyclone must reestablish itself, there being no chance for the existence of a real continental anticyclone. Because of the strong influence of Hann the true significance of the anticyclonic winds was not grasped by the members of that expedition, nor by Sir Napier Shaw, the director of the British Meteorological Office, who prefaced the report on meteorology by a statement consistent with Hann's idea of a shallow anticyclone. Meinardus has similarly interpreted results of the German expedition under Drygalski. When speculative ideas thus control the interpretations of observers it is no wonder that progress has been slow toward a proper understanding of our present climate. Such an understanding will be difficult enough if observation is made the basis for interpretation. In this connection I would remark that the existing ice-sheets probably hold the key to a proper solution of the climatic conditions of the glacial epochs. The striking difference between the polar and equatorial regions which prevails to-day is in sharp contrast to that of the non-glacial periods and represents the waning phase of the Pleistocene glacial epoch. The existing ice-sheets also are likely to throw light upon the method of growth and the movement of the Pleistocene ice-sheets. Rapid advance in our knowledge may be expected now that the aeroplane can be pressed into service and radio communications established.

It seems to be fairly well established that such astronomical factors as variations in the eccentricity of the orbit of the earth, the precession of the equinoxes and changes in the plane of the ecliptic, which at one time were considered by glacial students as of dominant influence in glacial epochs, are subordinate to the geographical conditions, and only at times supplementary to them. The recurrence of glacial stages in the Pleistocene epoch does not correspond, even rudely, to the calculated times of greatest eccentricity of the earth's orbit, nor was there an alternation of glaciation in the northern and southern hemispheres during a precession cycle. It is known that a glacial stage required a much longer period than half of the precession cycle. Attempts to fix glacial chronology in terms of these astronomical factors are without support and entirely misleading. It is not so easy to dispose of another astronomical factor—the variation in solar radiation. So far as known, this variation is very slight, but observations cover so brief a period that it can not be assumed that the full amount of variation is known. There is a possibility that this factor has had considerable influence on the climate. Should terrestrial factors, when fully evaluated, prove inadequate to account for the conditions attending a glacial epoch, it may be necessary to fall back on solar variation. But even then it may be incapable of demonstration.

The hypothesis of continental drift, made prominent by Alfred Wegener, was given consideration as far back as 1866, when Sir John Evans presented a paper before the Royal Society "On a Possible Cause for Climatal Changes," which is published in abstract in the *Geological Magazine* for that year. Wegener's hypothesis was made the subject of discussion in a symposium participated in by fourteen scientists and published by the American Association of Petroleum Geologists in 1928 in a volume of 240 pages. Wegener contributed a brief paper of five pages, designed to meet objections to the hypothesis and to call attention to recent geodetic surveys in Greenland that seemed to him to support it. He first presented the hypothesis in 1912 in *Petermanns Mitteilungen*. In 1915 he brought out a book, "Die Entstehung der Kontinente und Ozeane." This went to a second edition in 1920 and a third in 1922 and was translated into English in 1924. It appears to have met with favorable consideration by many European students, but it has not been so well received in America.

It is not my intention to go into the discussion of the Wegener hypothesis further than to indicate its irrelevancy so far as applied to the several stages of Pleistocene glaciation. It assumes a migration of the north pole from a position west of Greenland eastward over that island into the Arctic Ocean and then northward to its present position. It interprets the glaciation to have started in the northwestern part of the North American continent and extended eastward across the continent and Greenland into Europe. Meantime Greenland became separated from Scandinavia by a westward drifting, thus giving the Atlantic a northward extension. It fails to recognize that the drift sheets, both in North America and in Europe, show a repetition of glaciation at widely separated intervals, and that successive glaciations covered essentially the same parts of each continent. So far as the last two glacial stages in North America are concerned, the Laurentide ice-sheet made a growth from east to west, or in the reverse direction from that indicated by Wegener. Were continental drift made the dominant factor it would be necessary to carry the pole westward in our Illinoian and Wisconsin glacial stages. But in view of what is clearly established in

the glacial succession in Europe and America the Wegener hypothesis is purely fantastic and scarcely worthy of consideration in this connection.

The Wegener conception that the opening of the North Atlantic took place in Pleistocene time through a westward drifting of Greenland is opposed by the distribution of the Pleistocene isotherms. In last year's paper the following statement was made on this point:

> It is a matter of some significance, concerning the influence of planetary winds on the oceans in the glacial epoch, to note that the southern limit of glaciation on the European side of the Atlantic is 10° to 12° of latitude farther north than on the North American side, from which it may be inferred that the isotherms showed a difference in latitude on opposite sides of the Atlantic similar to what is found to-day. This relation shows clearly that the warm waters were driven northeastward across the north Atlantic by winds in the Pleistocene glacial stages about as they are to-day.

Wegener's claim that longitude determinations in Greenland support his view of continental drift, and show that it is still continuing, has been analyzed by Sir Charles Close, director general of the British Ordnance Survey, who finds that the differences are all within the limits of probable error and are not sufficient to prove westerly drift.[3]

Marsden Manson has brought out an elaborate hypothesis of geological climates, in a series of papers appearing in the *American Geologist* in 1899, based on the rôle of clouds as reflectors of solar radiation and of terrestrial radiation in conjunction with an assumed gradual waning of the internal heat of the earth. As a consequence of this cloud envelop it is inferred that, prior to the glacial epoch, the earth did not have the present sharply distinguished tropical, temperate and frigid zones. The conditions are most nearly realized at present in the equatorial rain belt, in which the land is maintained at the same temperatures as the neighboring ocean. While the solar radiation was greater in the low latitudes, the earth heat was not thus restricted, but was as great in high latitudes as in the equatorial region. The Permo-Carboniferous glaciation coincided to some degree with the present subtropical high-pressure belts, and it is thought that cold anticyclonic winds cooled the land most rapidly in those belts. After this the terrestrial radiation gradually diminished until the polar oceans became cold and the Quaternary glaciation followed. This glaciation was centered in the cold temperate belt of greatest precipitation, which it is thought was still overcast with a cloud belt. With progressive cooling of the oceans, the evaporation failed to supply sufficient moisture to maintain this cloud belt, and glaciation passed into the waning stage. It is still continuing in diminishing degree. The distribution of fossil plants and delicate marine organisms is such as to indicate that over long periods the present degree of climatic zoning was not in vogue. But it is a question whether cloudiness has been the controlling factor in giving this condition. The following statement concerning Manson's hypothesis is from the pen of C. E. P. Brooks, the British meteorologist:[4]

> The theory is interesting, but there are some insuperable difficulties. With warm oceans and an unbroken cloud canopy, the land surfaces, unless at a great altitude, would not be likely to freeze; the conditions are most nearly realized at present in the equatorial rain belt, in which the land is maintained at the same temperature as the neighboring oceans. "Cold anticyclonic winds" presuppose cooling by radiation. Even if under world-wide isothermal conditions the pressure distribution could remain unaltered, which is highly improbable, we must suppose either that the anticyclone would break down the cloud canopy, in which case the tropical sun would certainly prevent glaciation, or that the clouds would remain in spite of the anticyclone, in which case the descending air would not be cold. Finally, the moist conditions supposed by Marsden Manson to have prevailed during the warm periods are in direct opposition to the dry conditions demonstrated by the geological evidence set out in the Introduction.

It was pointed out by Brooks in the introduction to the volume just cited that the predominant features of the normal geological climate were warmth and dryness. "Deserts have apparently existed throughout geological time, but during most of the warm periods, and especially in the Mesozoic, they expanded greatly, extending from the sub-tropical regions far into the present temperate zones." It is probable that existing ice-sheets are responsible to a high degree for the present strong contrast in climatic zones. On this point Hobbs has expressed the following opinion:[5]

> The atmospheric circulation is given its vigor at the present time not alone through a pushing upwards of currents within the tropics as a consequence of excessive insolation within that region, but also by a pulling down by the refrigerating engines of the continental glaciers. . . .
>
> During both the Permian and the Pleistocene geological periods there were vast continental glaciers in addition to those which were located over the Antarctic and Greenland. Presumably there was glaciation of these areas since there were ice-sheets in lower latitudes. The in-

[3] *Geographical Journal*, Vol. 63, 1924, pp. 147–150.

[4] "Climate through the Ages," pp. 145–146.

[5] "Glacial Anticyclones: The Poles of the Atmospheric Circulation," pp. 168–169.

fluence of these vast domes of ice in stimulating a vigorous circulation must have been of the utmost importance, and the zonal distribution of climates should in consequence have been so much the more pronounced.

That the present zonal contrasts are connected with glacial climate is expressed in papers by leading American paleontologists which have appeared since Manson's hypothesis was brought out.[6] Of strikingly different attitude is the recent work by Köppen and Wegener, "Die Klimate der geologischen Vorzeit," Borntraeger, Berlin, 1924, which assumes that at all times in the history of the earth the same climatic contrasts have been exhibited. They also state that the changes of climate for a given region are to be interpreted as due to continental drift.[7]

A leading hypothesis based upon atmospheric conditions pertains to variations in the amount of carbon dioxide and of water vapor in the air, which was brought out by Arrhenius in 1896[8] and subsequently elaborated by Chamberlin.[8] It is suggested that the cold periods were characterized by a depletion of the carbon dioxide and warm periods were times when it was more abundant in the atmosphere. This depletion it is thought might be due to geological processes, the formation of coal and limestone being cited by Arrhenius, and the weathering of rocks made prominent by Chamberlin, as well as the agency of organisms. Variations of volcanic activity are also considered of importance. Chamberlin also developed an elaborate interpretation of the effect of the variations in carbon dioxide upon oceanic circulation, and showed that a reversal of the oceanic circulation might give rise to warm periods in high latitudes. This hypothesis has the advantage of being world-wide in its application and thus meets a need that is not met by hypotheses based chiefly upon local geographic influences. There is, however, considerable doubt expressed by meteorologists as to carbon dioxide having the influence attributed to it by Arrhenius and Chamberlin. Ångström maintains that the absorption effects attributed by Arrhenius to carbon dioxide are principally due to water vapor.[9] The water vapor content is determined by the amount of radiant energy received from the sun. Its variations are not, therefore, an ultimate cause for climatic variations. It has been pointed out by Humphreys that an increase in carbon dioxide could only affect the temperature by absorption at high levels in the atmosphere where water vapor is nearly absent, and he concludes that carbon dioxide can never have been an important factor in climatic variations.[10] A similar view is expressed by Brooks,[11] who grants that the quantity of carbon dioxide may have had considerable variation, but that these variations could not have had a great climatic effect, for the part of the terrestrial radiation taken up by carbon dioxide is almost completely absorbed by water vapor, and no increase in the amount of carbon dioxide could appreciably increase the total absorption.

It has been shown by several students that volcanic dust in the atmosphere is likely to have had a cooling effect, because of a scattering and reflection of solar radiations which it occasions. Benjamin Franklin suggested that the severe winter of 1783–84 might be due to great quantities of volcanic dust in the air. Observations on the eruption of Katmai in 1912 showed that the Katmai dust reduced the solar radiation reaching the earth by about 20 per cent., which if long maintained would lower the mean temperature of the earth about 10° F., an amount sufficient to initiate an ice-age.[12]

In view of the dissenting opinions among those who have given most attention to the effect of variations of the amount of carbon dioxide on climate it seems necessary to hold this hypothesis and that of volcanic activity as a factor in glacial climate open for further light.

One of the most significant contributions to the problem of glacial climate has been made by C. E. P. Brooks in his recent volume, "Climate through the

[6] See David White and F. H. Knowlton, "Evidences of Paleobotany as to Geological Climate," SCIENCE, n. s., Vol. 31, 1910, p. 760. F. H. Knowlton, "Evolution of Geologic Climates," *Bull.* Geol. Soc. Am., Vol. 30, 1919, pp. 499–566. Charles Schuchert, "Climates of Geologic Time," Smithsonian Report, 1914, pp. 277–311.

[7] "In diesem Buche werden die vorzeitlichen Klimawechsel unter den Voraussetzungen der Theorie der Kontinentenverschiebung behandelt, die hier als richtig angenommen wird" (page 1).

[8] Svante Arrhenius, "On the Influence of Carbonic Acid in the Air upon the Temperature of the Ground," *Phil. Mag.* (5), Vol. 41, 1896, pp. 237–276. T. C. Chamberlin, "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis," *Jour. Geol.*, Vol. 7, 1899, pp. 545–584, 667–685.

[9] Knut Angström, "Über die Bedeutung des Wasserdampfes und der Kohlensäure bei der Absorption der Erdatmosphäre," *Ann. der Physik* (4), Vol. 3, 1900, p. 720. See also Hann's "Handbook of Climatology," 1903, p. 399.

[10] W. J. Humphreys, "Physics of the Air," 1920.

[11] C. E. P. Brooks, "Climate through the Ages," 1926, pp. 132–133.

[12] For discussions of this subject see: C. G. Abbot and F. E. Fowle, "Volcanoes and Climate," Ann. Astrophys. Obs. Smithsonian Inst., 1913, and Smithsonian Misc. Coll. No. 29, 1913. W. J. Humphreys, "Volcanic Dust and Other Factors in the Production of Climatic Changes, and Their Possible Relation to Ice-ages," *Jour. Franklin Inst.*, Vol. 176, 1913, pp. 131–172. Also *Jour. Wash. Acad. Sci.*, Vol. 3, 1913, pp. 365–371. C. E. P. Brooks, "Climate through the Ages," 1926, pp. 133–136. H. Arctowski, "Volcanic Dust Veils and Climatic Variations," *Annals N. Y. Acad. Sci.*, Vol. 26, 1915, pp. 149–174.

Ages," noted above. He points out that glacial periods have occurred at times of high altitude of the land, while non-glacial periods are times of relatively low relief. He also expresses the opinion that in periods of low relief the barometric depressions, or cyclones, were less definitely developed than in periods of high relief and that storms were the thunderstorm type rather than long steady rains, such as are associated with the passage of barometric depressions. If the relief were sufficient in the district traversed by the cyclonic storms to cause precipitation as snow throughout the winter months, there would be a tendency to lower the temperature over the bordering seas and cause them to become ice-covered. When the ice cover reached a certain size the temperature on its edge would fall below the freezing-point of sea water, and it would continue to expand owing to the lowering of temperature which the ice itself introduced. By a series of mathematical calculations it is shown that a great climatic effect may follow such a covering of the polar oceans with ice, so it is merely a matter of getting the freezing process started. This might be brought about by only a slight general lowering of the earth's temperature. It is estimated that if the present average elevation of 2,500 feet were to be increased to 3,500 feet the effect on cloudiness, evaporation and area above the snow line would produce a lowering of temperature somewhere between 5° and 15° with a probability of about 9° F. While elevation is, by itself, and directly a very important factor in climate, it also causes, secondarily, changes in distribution of the land and sea, in the course of ocean currents, etc., all of which must be considered in accounting for the glacial epochs.

None of the hypotheses of purely terrestrial application appears to satisfactorily account for the great climatic fluctuations that characterized the Pleistocene glacial epochs. The glacial stages were interrupted by long intervals in which the temperature appears to have been similar to the present and the ice-sheets reduced to their present dimensions, if not more completely dissipated. It is difficult to picture geographic changes of sufficient influence to meet the conditions, though Brooks maintains that they need not be of great magnitude. He recognizes, however, that measurable climatic changes in historic time have had no basis in geographic changes and calls in extra-terrestrial influences to account for them. It is also difficult to picture changes in the amount of carbon dioxide resulting from variations in weathering processes or from volcanic eruptions of sufficient importance to meet the conditions. It is becoming increasingly apparent that some peculiar combination of several factors is necessary to account for the climatic variations exhibited in a glacial epoch. The key to this combination is yet to be discovered. It probably will be found when the factors affecting present climate are more fully understood.

As indicated above, it is probable that observational data on the existing ice sheets will aid in the interpretation of conditions attending the Pleistocene and earlier ice-sheets. At my request, Professor W. H. Hobbs has kindly outlined the lines of study that seem most desirable, as follows:

Extension of observational data on existing continental glaciers:

(1) Measurement of ice movement within the marginal portions.
(2) Determination of depth of ice to rock base by echo method.
(3) Determination of snow density in all sections and structure beneath by the Mt. Rose method.
(4) Determination of time and place of snow precipitation over the ice-sheet and redistribution as drift snow.
(5) Evidences of fusion of surface snow on the ice-sheet.
(6) Measurements of outward radiation from the surface of the ice-sheet compared to land (bare or snow covered), to sea water and to sea ice.
(7) Exhaustive study of the glacial anticyclone from weather stations upon and about the ice-sheet with aid of pilot and sounding balloon studies of the upper air. A station within the interior to be compared to aerological stations about, but close to, its borders. Variations in the energy of the mechanism, with corresponding variation in the dimensions of the model, should be studied (winter and summer seasonal changes).

For such studies the Greenland ice-sheet offers better opportunities, because of its greater accessibility, and its simpler model. It seems to lack the interior mountain ranges, which greatly complicate the problems of the Antarctic. Aerological stations about but above its borders should tell as much.

World-wide Correlations of Glacial Stages

While it is coming to be generally recognized that the Pleistocene glaciation was interrupted by relatively warm interglacial stages, much is yet to be done to clear up the correlation of glacial stages in the different areas of glaciation. There is still only a partial correlation of the stages in the Scandinavian field with those of the Alpine field. There is similar uncertainty as to correlations between certain drifts in the eastern and western parts of the Laurentide area of glaciation in North America. It will be well to settle the correlations within each of these continents as a preliminary to full correlations between the continents. The clearing up of these correlations between Europe and America and between the glacial stages of the northern and southern hemispheres is

probably merely a matter of more detailed study of the glacial and interglacial deposits in the several fields.

LENGTH OF PLEISTOCENE GLACIAL AND INTERGLACIAL STAGES

The length of time involved in the Pleistocene glacial epoch, with included interglacial stages, has been variously estimated, though there seems to be a general consensus of opinion that at least a half million years would be required for the changes wrought since the earliest drift of Europe or of North America was laid down. Some students are inclined to double the time and put the beginning of the glacial epoch back a full million years. The main basis for estimates of the time involved has been the amount of erosion and degree of weathering that have taken place in Pleistocene time. The length of interglacial stages has been determined by a comparison of the erosion and weathering of the drift of a given glacial stage with that of the next succeeding glacial stage, allowance being made for the probable time involved in the glacial stage. This allowance, however, is a difficult matter to estimate. To properly measure a glacial stage one must determine the time involved in the advance to the culminating position, and the time involved in the development of moraines formed in the course of the waning part of the glaciation, as well as the time between successive moraines. The measurement of the varves, or annual deposits of laminated clays, introduced by De Geer in Europe and North America, and carried on widely by Antevs in the United States and Canada,[13] pertains to the time involved in the recession from one moraine to another, but not the time involved in forming a moraine. In some cases also a readvance of the ice border has buried part of the record of the recession. An entire glacial stage is likely, therefore, to require several times as long a period as that measurable by the varves. The time involved in the recession of waterfalls, such as Niagara and the Falls of St. Anthony, also covers only a small part of the waning stage, and the estimates are of chief value in giving a measure of postglacial time. From these estimates, combined with a study of the erosion of the latest drift sheet, it is calculated that it requires about 10,000 years to effect one foot of average erosion of the till. On this basis the Illinoian till, with an estimated erosion of fifteen feet, would be 150,000 years old, and the Kansan till, with an estimated erosion of fifty feet, would be 500,000 years. These estimates are perhaps of some value in giving a rough approximation as to the time involved, but should be supplanted by more refined methods of measurement. It is also important, as above noted, to work out some method for estimating the time involved in the glacial stage, and determine what proportion of the Pleistocene glacial epoch was under glacial conditions and what under the relatively warm interglacial conditions. Much is yet to be learned as to the periodicity of moraine development. It also is a matter of importance to determine why certain districts have a fuller series of moraines than neighboring ones. Our studies have shown how the ice-sheet distributed moraines but not why it did so.

[13] Gerard De Geer, "A Geochronology of the Last 12,000 Years," Compte Rendu du XIth Congrès Géologique International, 1910, pp. 241–253. The studies were begun by De Geer in 1879 and the results appear in papers in the Swedish language beginning in 1882. Ernst Antevs, "The Last Glaciation," Am. Geogr. Soc'y, Research Ser. No. 17, 1928, 292 pages. See also "The Recession of the Last Ice-sheet in New England," Ditto, No. 11, 1922, 120 pages. Also Canada Geol. Survey, Memoir 146, 1925, 142 pages.

RELATION OF GLACIATION TO PLEISTOCENE DIASTROPHISM

It is well established, through a study of tilted shore-lines, that the interior part of the glaciated districts in northeastern North America and in northwestern Europe have risen several hundred feet since the ice-sheet melted from them. They appear to have only partially recovered an altitude they possessed prior to the glacial epoch. The depression of the land is referred to weighting of the ice-sheet, and its recovery to relief from weighting. The presence of thick deposits of drift and of bodies of water in the lake basins is thought to have influence in preventing a complete recovery of the preglacial altitude.

The study of the shore-lines of the glacial lakes has brought out a marked lack of correspondence between the area of ice weighting and the uplifted area. It is found that the shore-lines show no tilting in the south half of the Lake Michigan basin, nor in all the Erie basin except the northeast end. Yet the ice-sheet extended far beyond the limits of these basins, and must have been thousands of feet thick in the parts unaffected by tilting. These studies and studies in other basins occupied by glacial lakes have shown that the uplift extends only a short distance beyond the Precambrian lands into the lands covered by Paleozoic formations. There appears to be a closer correspondence with the border of the Precambrian lands than with the amount of ice weighting. It appears that the ice weight was insufficient to cause such a depression in the stable areas covered with sedimentary Paleozoic formations as it was able to produce in the highly eroded Precambrian areas. There is thus an interesting problem in the relative stability of an area of great erosion and one in which sediments have accu-

mulated. A recent paper by Bowie deals with this matter, as indicated by the following quotation:[14]

> The term residual rigidity is frequently used in discussing earth problems. A material that is said to have residual rigidity is supposed to resist deformation unless the force exerted on it approaches the elastic limit of the material. In this sense the residual rigidity is equivalent to the term strength. . . .
>
> It would seem probable that the strength of the crust under the sedimentary zone should have been augmented rather than decreased by the consolidated sedimentary material. . . .
>
> If there are any weak zones of the earth's crust it would seem that they underlie the regions which have undergone great erosion.

In this connection attention is directed to an erroneous map[15] prepared by a leading American glacialist, in which isobases of postglacial uplift are made to correspond to an estimated thickness of the ice-sheet in the region east of the Mississippi River, thus disregarding the results previously published of observations showing that there is no such close correspondence. No progress can be made where office speculation is substituted for or given more weight than field studies.

The Loess Problem

Although loess is now generally recognized to be a wind deposit, it seems to have been developed under conditions related to if not dependent upon glaciation. In America its distribution along main watercourses, such as the Mississippi and Missouri valleys, and its northward limitation on the border of the glacial deposits led to an early interpretation that it was transported by streams heading in the ice-sheet. The Mississippi valley was supposed to have been down to a level low enough to give rise to fluvio-lacustrine conditions. Similar views were held by European students as to the distribution of the loess there. It soon became evident, however, that the distribution of the loess and also its fossil content favor deposition on the land, and that it was redistributed by streams down the valleys. It has also been determined that the greater part of the loess of the Mississippi basin was brought in from the semi-arid plains to the east of the Rocky Mountains, only a minor part being derived from the glacial deposits. The European and Asiatic loess deposits were also found to have been derived mainly from semi-arid districts.

The chief problem now seems to be in reference to the time of deposition of loess in relation to glacial stages. By some students loess deposition is considered a forerunner of glaciation and to some degree dependent on the conditions that produced glaciation. In places it seems to correlate with the culmination of a glacial stage. Such is the case on the early Wisconsin drift in Illinois, there being loess on the unweathered surface of the drift near its border. The waning part of a glacial stage seems to have been characterized by more humid conditions than attended the culmination, the glacial drainage being more vigorous. Such being the case it probably would be an unfavorable time for loess deposition. The molluscan fauna of the loess is very similar to the present fauna of the region and thus of a more temperate type than seems consistent with a glacial stage. This has been the main reason for giving it an interglacial position. Much, however, is to be learned as to the degree to which such a fauna would accommodate itself to climatic changes.

It is well established that there have been two if not three periods of extensive loess deposition on or near the border of the drift sheets in Europe and in North America. It will be of importance to determine whether these periods can be correlated on opposite sides of the Atlantic. Such correlations will depend upon the success achieved in correlating the drift sheets with which the loess deposits are associated.

The characteristics of loess are so different from those of other wind deposits as to raise the question of its mode of transportation. It possesses a homogeneity that is strikingly in contrast with the heterogeneous material now swept by the wind over the surface of the ground. It also is of much finer material than dune sand. It seems to be a dust that would be susceptible to long-distance transportation, and its wide distribution as well as its composition support such a history. It appears to have settled on the land which it covers and not to have been swept across it. In general, it forms a continuous sheet, allowance being made for subsequent erosion. But on the outskirts it is more patchy. Some of this patchiness may be due to subsequent sweeping by surface winds. There are strips of loess-free land alternating with loess-covered strips in such manner as to indicate much diversity in wind effect, the loess-free strips having been exposed to strong wind action from which the loess-covered strips were protected, perhaps through some difference in vegetation. Such features are well exhibited in northeastern Iowa, near the border of the Iowan drift. They may prove to be related to wind action from the Iowan ice.

Relation of Man to the Glacial Epoch

The evolution of man from lower animal forms appears to have taken place at least as far back as

[14] William Bowie, "Zones of Weakness in the Earth's Crust," SCIENCE, n. s., Vol. 70, 1929, pp. 589–592.

[15] *Bull. Geol. Soc. of America*, Vol. 29, 1918, p. 202, fig. 1.

the early part of the Pleistocene epoch. Some students hold the opinion that the human stage was reached at a much earlier time. The skeletal remains of man imbedded in deposits of early and middle Pleistocene age in Europe, the Heidelberg, Sussex and Neanderthal remains, are of a crude type compared with those of the late Pleistocene Cro-Magnon race and suggestive of a lower order of mentality. It is a matter of importance to determine whether the inhospitable conditions of the glacial epoch stimulated mental development or had instead a brutalizing effect. Did the cruder races become exterminated under the adverse conditions attending glaciation, or instead did they develop into the superior types of men that held possession of Europe in late glacial and early postglacial time? It is a question whether the highly developed Cro-Magnon race was evolved under these adverse conditions or whether its development took place in a part of the earth where the climate was more genial and from which the race migrated into Europe as climatic conditions there became more favorable. As the race showed a strong artistic bent its origin may be determined by a wide-spread study of the works of art of primitive people.

There appears to be as yet no clear evidence of human occupancy of the American continent during the glacial epoch. There have been frequent reports of the finding of chipped implements in the glacial gravel, and in the loess, but examination into the evidence by archeologists and geologists has led to the conclusion that the implements had been recently imbedded.

Problems of Glacial Oscillations

Attention was called in the discussion of the duration of the glacial epoch to the lack of knowledge of the length of time involved in forming a terminal moraine. There is also a lack of knowledge as to the cause for the oscillations of the ice border so clearly shown in the waning part of a glacial stage. Many moraines show a readvance of the ice border. These readvances are separable into major and minor ones. The moraines forming the limits of the middle Wisconsin and the late Wisconsin drift illustrate the major class and mark a pronounced readvance of the ice border, also considerable shifting in the direction of the ice movement. It is on the basis of these readvances and realignments that the middle and late substages of the Wisconsin glacial stage have been recognized and named. The minor readvances amount usually to but a few miles, but like the major ones they serve to show that there was some factor in operation to intensify glacial conditions at certain times during the waning part of the glacial stage. The geographic factors are unlikely to have varied in such way as to cause this oscillation of the ice border. Nor do atmospheric factors, such as variations in the carbon dioxide or in the presence of volcanic dust, seem likely to have played an important part in intensifying glacial conditions. Seemingly it must be a climatic cause; yet the nature of the climatic change remains a problem.

Origin of Drumlins, Kames and Eskers

Widely different interpretations of the origin of these features are found in the glacial literature, European as well as American. It is probable that some drumlins are due largely to erosion by the ice-sheet, while others are of constructional type, built up beneath the ice-sheet. Kames are likely also to have had more than one method of development. In some cases they are evidently the product of drainage in connection with an active phase of the ice border, for they show disturbed stratification, apparently due to an advance of the ice border. This has given them greater relief and incorporated till with the gravel. In other cases where composed entirely of gravel and sand in undisturbed position there may have been a stagnant or stationary position of the ice border. The long gravel ridges known as eskers, while probably formed by streams flowing within or beneath the ice-sheet, can scarcely have been formed from end to end at one time, even though the ice-sheet had become stagnant. The upper ends are probably of later development than the lower, there being a lengthening headward with the recession of the ice border. Long eskers are in many cases diversified by a plexus of ridges, occurring at short intervals. The plexus may mark the starting of a new system of drainage within the ice-sheet not far back from the ice border. Detailed study should clear up the manner in which these ridges were formed.

Origin of Gumbotil

In a part of the Mississippi drainage basin, chiefly in Iowa, Missouri and Illinois, the surface of the Illinoian, Kansan and pre-Kansan drifts, where exceptionally flat, is characterized by a gummy gray clay to which Kay has given the name gumbotil.[16] It is interpreted by him to be mainly the product of weathering of the surface portion of the drift, and its thickness to be a measure of the time involved in its development. It has a thickness of two to five feet on the Illinoian till where best developed and a greater thickness on the Kansan and pre-Kansan tills. Its greatest thickness is on the flat divides on the Kansan drift of southern Iowa and northern Missouri, where it commonly is about eight feet and

[16] G. F. Kay, SCIENCE, n. s., Vol. 44, 1916, pp. 637-638.

occasionally twelve feet or more. It is singularly lacking in coarse pebbles, even in places where the underlying drift carries coarse pebbles of quartzite which would be likely to withstand dissolution for a much longer period than is embraced in the Pleistocene glacial epoch. There are small quartz pebbles in the deposit at all levels, but large pebbles are only found close to the underlying till. The thickness of the gumbotil decreases in passing northward, but it has been found on the Kansan till in northeastern Iowa beneath the Iowan till. It is, however, not so thick there as it is beneath the Illinoian till in southeastern Iowa, or beneath the loess in other parts of southern Iowa and in northern Missouri. The gumbotil on the Illinoian drift is thickest at the western edge of the drift, in southeastern Iowa and western Illinois. It is inconspicuous or wanting in eastern Illinois, Indiana and Ohio. It has not been noted on the Illinoian drift of Pennsylvania and New Jersey. Nor has it been observed on the Jerseyan drift in those states.

The restriction in distribution and the variation in thickness on a given drift seem to indicate that there is some factor other than mere lapse of time that governs the development of gumbotil. The restriction of coarse pebbles to the base of the deposit raises the suspicion that it may be only to a moderate degree the result of weathering of the till. The small pebbles in its upper part may in that case have been brought up into it by crayfish, as is the case in poorly drained parts of the loess. There is perhaps significance in the fact that the gumbotil shows variations in thickness that correspond in some degree to variations in the thickness of the loess. The thickness of the gumbotil thus may be found to depend partly upon wind deposition. If the wind brought in material at a very slow rate it might become weathered to a gummy condition, as the fine material in the till is supposed to have been. The deposition of the loess was probably at a sufficiently rapid rate to escape conversion into such a condition. Whatever its origin the gumbotil appears to have required a period of considerable length for its development, for its material shows throughout an advanced degree of weathering.

Kay has expressed the opinion that the gumbotil was formed under conditions of low altitude unfavorable for the erosion of the drift and has assumed that its development was nearly completed prior to a hypothetical uplift of the land that gave favorable conditions for erosion.[17] On this supposition a long period must be given for gumbotil development and another long period for the erosion of the drift, as indicated in the following quotation:

[17] *Bull. Geol. Soc. Am.*, Vol. 27, 1916, pp. 115–117.

After the gumbo plain had been developed by weathering processes on the Kansan drift plain, diastrophic movements seem to have occurred, the plain having been elevated to such an extent that erosion became effective and valleys began to be cut into the gumbo plain. Erosion of the gumbo plain progressed to such an extent that some valleys were cut to a depth of more than 150 feet before grade was reached and a mature topography was developed. There are now only remnants of the original gumbo plain, the most conspicuous of these being flat, poorly drained areas, known as tabular divides.

It is a matter of considerable importance in estimating the time since the Kansan stage of glaciation to determine whether conditions favorable for the erosion its drift displays were operative as soon as the ice uncovered it or only came into operation after a long period as Kay has suggested. It seems unnecessary for the gumbotil to have been completely developed before erosion began if the land had an attitude favorable for erosion from the beginning. The gumbotil, it would seem, might have been developing on flat areas throughout the time in which erosion was active, until prevented from further development by the deposition of a sheet of loess over it. The loess deposition is known to have taken place after the Kansan drift had become greatly eroded. So far as I am aware, Kay has produced no evidence that would clearly establish the uplift of the Kansan drift area which he has postulated, but has merely made the assertion that diastrophic movements seem to him necessary to succeed gumbotil development. This matter concerns Illinoian as well as Kansan gumbotil development and naturally leads to the consideration of drainage conditions in the Mississippi basin in the Kansan and Illinoian stages of glaciation.

Conditions of Glacial Drainage in the Mississippi Basin

So far as can be determined after allowing for the great amount of erosion that the Kansan drift and its outwash have experienced, there was very little material carried from the border of the Kansan ice-sheet into the valleys that led away from it. There is a moderate amount of gravel in the Mississippi valley at Hannibal, Missouri, where the Kansan drift border passes across the valley from Illinois into Missouri, and also at a few points below, which appear to indicate that conditions were favorable for the discharge of water down the valley while the ice was melting. The Missouri valley in Missouri was largely outside the Kansan drift border, but the drift extends south of the valley in Saline County, and there considerable sand and gravel are present along the edge of the drift and interbedded with the till. Below there, in Cooper County, the valley was filled suffi-

ciently to cause a temporary flow of the Missouri through a low passage only two to three miles south of the river. These features combine to show that the ice-sheet was not terminating in ponded water in either of these main valleys. There is, however, such general scarcity of sand and gravel in the Kansan drift in its entire area of exposure, in Missouri, Kansas, Nebraska, Iowa, Wisconsin, Minnesota and South Dakota, as to indicate that there was very weak flow of water from its border as it was melting back across these states. So far as known also no morainic ridges were formed in the course of its retreat. It becomes, therefore, a matter of interest to determine the cause for these conditions. The lack of moraines suggests a steady recession of the ice border, without such oscillations as characterized the Wisconsin stage. The lack of outwash, if not due to poor drainage conditions, may perhaps be accounted for by the slow rate of melting of the ice.

The Illinoian drift also is remarkably free from outwash over a considerable part of its area of exposure, from southwestern Ohio westward into southeastern Iowa. But along its border in southeastern Iowa there is a well-defined channel opened by the temporary Mississippi River at the time its present valley between Clinton and Ft. Madison was occupied by the ice-sheet. There is a deposit of sand and gravel of probable Illinoian age in the Mississippi valley below Keokuk. But none has been noted on the border of the Illinoian drift in southeastern Iowa or in Illinois. The temporary Mississippi channel is nearly free from sand and gravel, though it appears to have had free discharge. This gives rise to the question whether the temporary Mississippi River was mainly carrying the drainage from the outlying land and receiving very little water from the melting Illinoian ice-sheet. In that case the rate of melting of the ice was probably very slow. The Illinoian ice-sheet formed several definite morainic ridges as it was melting back across Illinois and has a definite morainic ridge along most of the drift border in Illinois, Iowa and southern Wisconsin. It also carries gravel in the form of kames and eskers over much of its area of exposure in these states and farther east. There is a change from very scanty outwash to a conspicuous outwash at the Scioto valley in southern Ohio, and this condition is maintained from there eastward to the Allegheny Mountains and is also found on the Susquehanna valley in eastern Pennsylvania. Inasmuch as the Ohio valley was covered for a long distance by the Illinoian ice-sheet at its culmination, in the part below the mouth of the Scioto River, it is probable that the vigorous Illinoian drainage on the Scioto and streams farther east came after the ice-sheet had vacated the Ohio valley.

The Iowan drift is characterized by very scanty outwash in northeastern and northwestern Iowa, but has conspicuous outwash into Big Sioux valley and several of its eastern tributaries in South Dakota. The scantiness of outwash in Iowa seems not to have been due to obstructed drainage. It may prove to be due to the slow rate of melting of the ice.

Outwash is very scanty in connection with the moraines of the early Wisconsin drift in Illinois, Indiana and Ohio, but is conspicuous in connection with the moraines of the middle and late Wisconsin drift in these states. The difference is not attributable to less favorable conditions for the discharge of water from the early Wisconsin moraines. In this case, as in the older drifts, there may have been too slow a rate of melting to give vigorous flow from the ice border. The early Wisconsin moraines carry a thin deposit of silt loam of loess-like character, and this, as already indicated, appears to have been deposited before the surface of the moraines had suffered perceptible weathering or erosion. Perhaps this silt loam was carried up on the edge of the ice-sheet by wind blowing from the loess-covered district outside, in which case it would tend to protect the ice from the sun and lower the rate of melting. It is to be expected also that the rate of melting of the ice-sheet when at its culminating position would be slower than in the waning stage, when warmer conditions had set in.

THE IOWAN DRIFT PROBLEM

The Iowan, or third drift of the region west of the Driftless Area of the upper Mississippi valley, is of such a problematical character that the students who have examined it have been unable thus far to come to an agreement as to its place and rank in the glacial series. It is a very thin and somewhat patchy deposit, whose eastern limits in Iowa and Minnesota are so difficult to define that there is no agreement as to its extent. The western limits, in northwestern Iowa and northward into South Dakota, are better defined, being marked in places by a weak morainic ridge. The western part was interpreted by Macbride, and also by Leverett and Sardeson, to be referable to the Wisconsin stage of glaciation. But later studies by Leverett and by Kay and Carman have led them to consider it a probable correlative of the Iowan drift of eastern Iowa. This interpretation is based upon the degree of weathering that the drift exhibits, and the presence of a loess deposit on a considerable part of its surface. It is also very thin and patchy like the eastern area of Iowan drift. In both areas it overlies a greatly eroded Kansan drift, and is accordingly interpreted to be much younger than that drift. In constitution it is very similar to the Kansan drift, and probably was largely derived from it.

The principal question now in dispute is the relation of the Iowan to the Illinoian drift. Chamberlin and Leverett in recent years have referred it tentatively to the same glacial stage as the Illinoian drift. But Kay and his associates on the Iowa Geological Survey and Alden of the U. S. Geological Survey hold to an early idea that it is the product of a distinct glacial stage standing between the Illinoian and the Wisconsin. They grant, however, that there does not seem to be any equivalent of the Iowan drift in the district east of the Mississippi valley. They thus restrict the Illinoian drift to the Labrador part of the Laurentide field of glaciation, and the Iowan to the Keewatin part, which seems a very doubtful and unnatural restriction.

The reference of the Iowan drift to a later glacial stage than the Illinoian is based by these students on the lack of a gumbotil deposit on its surface, such as is found on neighboring parts of the Illinoian drift. They also maintain that the erosion and weathering and especially the leaching of lime is less on the Iowan drift. Recently Kay has announced the presence of a loess deposit on the Illinoian drift that seems to him to correlate with a loess that underlies the Iowan drift. The absence of gumbotil on the Iowan drift seems to be due to a lack of favorable conditions for its development rather than to a lack of time. It is hoped that further field study may clear up the remaining points of difference.

CONCLUSION

From what has been outlined it will be seen that there are problems of various kinds awaiting solution. These problems call for training in various lines. There will be work for students of various degrees of ability. But, as remarked by one of my associates, it will require native ability, thorough training and a steady scientific aim to clarify the main problems of glacial geology.

A NEW SCIENCE[1]

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The most ancient art—and still an art—emerges into the most modern science, one that began to develop with the "Machine Age" and never will be completed.

We need science in education, and much more of it than we now have, not primarily to train technicians for the industries which demand them, though that may be important, but much more to give everybody a little glimpse of the scientific mode of approach to life's problems.—R. A. MILLIKAN.

"NECESSITY—thou best of peacemakers, as well as surest prompter of invention," said Sir Walter Scott. Necessity has often been described as the mother of invention, but there have been other parents such as desire and even accident. Whatever the economic urge behind invention may have been, it is clear that to-day science is the father of both discovery and invention so far as method is concerned. What have been the forces which have propelled individuals toward new goals? The search for the beginning of an idea usually leads far back of the date of the invention, and it is difficult to place a historical finger on the individual who originated an idea. National urges and movements are much easier to trace and in the long run are as important in measuring the causes of and steps in discovery and the resulting influences on the social organism.

[1] Address of the retiring vice-president of Section M—Engineering, American Association for the Advancement of Science, Des Moines, December, 1929.

Just what was the motive force five thousand years ago in the Nile Valley? A desire for a form of immortality led to the construction of the tombs and pyramids of Egypt. The temples of Thebes, Amenra, Edfu, Luxor, Memphis, Baalbeck and others were erected to perpetuate a cult. They represent a national skill, but not a national culture, as the erection of such monuments was by the decree of a despot and at the expense of thousands of slaves.

Greece was a democracy, and the national search for beauty gave us the Parthenon and the attendant philosophy of Plato. Not less impressive were certain scientific gifts contributed by Greek culture of which much less is said.

Religious zeal also gave us the Gothic period, which was the first architectural style to emphasize the vertical line. The cathedrals of Bourges, Chartres, Amiens, Rheims, Notre Dame, Salisbury, Wells, York, Lincoln, Cologne and Milan represent a wide-flung culture and a reborn skill.

Whatever have been the incentives to progress and whatever scale of intelligence may have been reached, there have been limitations to the skill or productivity of nations, and the most obvious has been the tools which they used. The implements of primitive man were the product of hunger, fear, love and his environment, but the later national advances were limited by the tools available, and tools have been a product of science as the latter has fabricated new materials and

discovered new principles. The greater the advancement of a nation, the more it is dependent on the contributions of science.

The tools of the Egyptian period from 4000 to 1000 B. C. were of bronze and included straight and circular saws, solid and core drills, chisels, hammers and axes for dressing timber and stone. Their skill as workers in stone was limited by the materials and tools which they possessed. The Greeks attained even finer expression with tools of the same materials, and their craftsmanship as designers and sculptors was acquired and perfected in a comparatively short period of national history.

Invention could offer very little improvement in tools and skill until long after the Christian era began. Roman ruins reveal bronze as the metal most frequently used, and the best Roman art was transplanted from Greece.

Roman aqueducts are monuments to the ability of early engineers who worked with simple tools. Aqueducts and roads were among the earliest utilities constructed for public benefit. Egypt had its irrigation canals and pyramids; Greece had supplied water to Athens by gravity, but Rome was conspicuous for its early engineering works which were constructed in the face of great obstacles. Such knowledge as there was of column, beam and arch was empirical—the product of experience rather than of science. The first circular arch of which we have knowledge is Egyptian and dates from about 1550 B. C. It was formed of four courses of brick totaling four feet in thickness and was eleven feet in span. The dome belongs to a later period, about 440 B. C., and was probably of Etruscan origin.

For a thousand years, skill remained at or below the Greek level. Few inventions were made, and inventions are ideas at work.

Let us trace a few of the most impressive inventions and discoveries of a scientific character and their influence on knowledge and skill in the use of it.

Astronomy began with the earliest civilization. The Egyptians, during their most productive period, had a calendar and a system of simple surveying, and they located the axes of some of their temples by stellar or solar observations.

But the Greeks left much more to admire. Pythagoras pronounced the earth round. Anaxagoras first stated the theory that the sun, moon, stars and meteors were of a common substance from a common source—"one of the most marvelous feats of human intelligence," says Henry Smith Williams in his "History of Science."

Ptolemy was a transplanted Greek who first discovered the solar system as such, though in error in locating the earth as the center. Little was possible in geography or navigation until the truth about the solar system was discovered.

Archimedes, the inventor of the screw pump, may be called the father of the science of mechanics. Previous to his time (the third century B. C.) water was raised by buckets operated by hand for irrigation and domestic use. His was an important contribution as it involved an analysis of the screw. He first formulated the principle of buoyancy and determined the relation between a sphere and a circumscribing cylinder by experiment and analysis.

There followed over ten centuries fallow in the field of mechanics. Galileo then corrected the theories of Aristotle on falling bodies, announced the principle of forces in equilibrium, of inertia, studied the telescope, the earth's motion and the pendulum. Newton followed immediately with his well-known principles.

The birth of the science of mechanics dates from Archimedes, but its adolescence lasted over fifteen hundred years. Its renaissance was accompanied by a galaxy of contributors, chief of whom were Kepler, Huyghens, Descartes, Maxwell, Rankine.

The study of anatomy, no doubt, began in prehistoric times, but it is not until the time of Pythagoras and Aesculapius that the science began to emerge. The function of heart, veins, arteries and the brain were contemplated, and "the four elements, fire, water, earth and air, were made the basis of all organisms," then little progress was made for two thousand years, until Harvey discovered the circulation of the blood, oxygen was studied by Priestley about 1774 and, beginning with Pasteur in the last century, came the rise of biology. The Greeks considered the atom, but this field of science was closed to them and to their successors until the microscope was produced.

Aristarchus and Hipparchus made real studies of the size of the sun, moon and earth, but their apparatus was inaccurate. Hipparchus is credited with the first pronouncement of the precession of the equinoxes, although the fact of such a movement must have been known to the Egyptians. Ptolemy followed Hipparchus but failed to accept the sun as the center of our solar system.

There was experimental knowledge at an early date of the effect of heat on air, but Hero, of Alexandria, was the first, so far as we have record, to use heat to do work. His inventions were toys rather than devices for relieving men of labor, and to them perhaps too much importance has been attached. Nevertheless, tremendous significance must be assigned to the Greek contributions to early science.

The Romans contributed to geography, medicine and law but left no such impress on science as the Greeks.

During the ten centuries following the opening of the Christian era a few contributions to science were made by Arabic and Roman scholars, but the period is comparatively sterile. Science was throttled; skill was limited. The rotundity of the earth was not accepted and a true concept of the solar system was not vigorously sponsored, though it had been suggested by both Greek and Alexandrian searchers.

In the thirteenth century, advances were made in medicine. Universities were founded in Italy, France, England and Germany. Roger Bacon reviewed the learning of certain Arabians and pronounced in favor of a scientific search for truth.

It was not until the fifteenth century that master minds emerged from the dark ages. Columbus, Leonardo da Vinci and Copernicus were born in this period. Columbus proved what Ptolemy and his followers had pronounced, that the earth was round. Leonardo added to the proof that the earth moved and the sun did not. He experimented with steam much as Hero had done, but made no material advance. He observed many facts of nature and formulated a few principles which marked an advance in the field of geology and optics. His accomplishments as an engineer, military genius and artist are much more impressive than his contributions to science.

Copernicus was a German who studied in Vienna and Rome. He taught mathematics and for thirty or more years considered the Ptolemaic theory. He did not publish his famous book addressed to the pope until about the time of his death.

Tycho Brahe contributed to the advance of astronomy but rejected a part of the Copernican theory.

Kepler, a sixteenth century observer, successfully defended the theory of Copernicus and announced his three laws concerning the variation in speed and in the distance of the planets from the sun as they traveled their elliptic orbits.

At about this same time, the first systematic studies of magnetism were made by William Gilbert in England. Galileo believed in him, and later Priestley called Gilbert the father of modern electricity. He coined the phrases, "north and south poles," "electric force" and "electric attraction." He distinguished between magnetism and electricity and made numerous experiments.

Alchemy and astrology had existed from the beginning, handmaids of ignorance, superstition and evil. They throve until about the time of Elizabeth, when advances in the search for facts began to cast grave doubts on them. Progress in anatomy was made by a considerable number of investigators leading up to Harvey, who first understood the blood circulation and the action of the heart. Closely following came the discovery of bacteria by Leeuwenhoek, in 1683.

These two discoveries laid the foundation for a science of medicine which, however, grew very slowly. The scientific method was applied by a few, such as Descartes and Leibnitz.

Boyle, Hook, Huygens, all added to the refinement of physics. Newton formulated his laws of motion and of gravitation, studied the spectrum of light, the nature of color, refraction and reflection.

A very large number of investigators followed Gilbert and contributed something to our knowledge of static electricity. It was, in fact, applied in medicine as early as 1743. William Watson, of England, appears to have described his experiments, and his descriptions were read by Benjamin Franklin. He and his coworkers began a long series of very productive experiments, first with the Leyden jar, from which he constructed a battery. Several had suspected the identity of electricity and lightning, but Franklin was the first to prove it.

A scientific basis for the development of an electrical science was laid by Faraday, Henry and others in the next half century, and close on its heels came one of the most spectacular changes in industry and society.

James Watt condensed steam in a more efficient way and his successors improved the art. Rankine, Carnot, Joule and others developed the science of heat energy and showed that heat *is* energy. Without this science our present social organization would be impossible.

May I emphasize that in nearly all fields the development of a science has been followed quickly by applications which have had great significance?

We have developed the science of the infinitesimal, of the infinite and of nearly every phenomenon in the earth beneath, the waters thereof and the heavens above so far as they relate to matter, force and motion. The changes resulting from this new knowledge have transformed the environment of man though they may have influenced his character but little. We find ourselves in the midst of an industrial civilization which has removed physical limitations and reduced manual burdens. This age of men and machines is hailed as a savior and condemned as a destroyer. Obviously, the machine itself is guiltless. If it serves, then the designer, inventor, manager are to be commended. If the machine is a curse, then it seems that the person or persons responsible for the abuse are guilty. The machine can do no wrong, can break no laws. Laws may abolish certain tools or machines because they are to be used for evil purposes just as men are removed from society because they are dangerous.

The science which has developed the materials and instruments of this age of power has been followed by

applications which were impossible before. The art has followed the science rapidly. The skill of men has been equal to the materials and tools provided.

Science has developed a law of action for nearly every force known to man, but there has been no accepted science of man-power, no science to serve as a guide to the management of men. Numerous principles have been proposed commencing with the Biblical injunction to "Do unto your neighbor as ye would that he should do unto you." The psychologist has studied intelligence, nerve action, inhibitions and complexes. The economist has studied trade, wealth and cycles of various kinds. The social scientist has dealt with masses, movements and legal restraints. None of these has attempted to develop either a theory or a practice of industrial operations, *i.e.*, a science of management.

The earliest important contribution to a science of directing and using human energy was by Frederick Taylor, who applied the scientific method of measurement to tasks and approached the problem of wages and incentives in the same manner. Gantt, Barth, Emerson, the Gilbreths and many others have contributed to this scientific achievement of the twentieth century. It has been summarized and formulated by Mr. L. P. Alford in a paper presented to the American Society of Mechanical Engineers in 1926, in which principles are laid down for handling materials, the product and men. The laws of leadership are capable of broad application even to colleges and universities. The law of responsibility, the law of exceptions, the law of task and wage incentives, the law of productivity, the law of acquiring skill are fundamental precepts, the recognition of which lays a firmer foundation on which to build administrative skill.

In engineering education recently nothing has attracted more attention than instruction in the field of human relations and industrial proficiency. The personality of the student as well as scholarship is being considered, and the approach to industry is becoming rationalized. Sentimentalism is being replaced by facts and preparation. The induction process begins in college and continues well into industry. Systematic study is replacing a *laissez faire* attitude, and something of the scientific approach is being made. Enlightened management is no less an art but more of a science, and so far as it is a science we may not ignore it entirely in our engineering instruction. The methods employed may differ widely but the purpose is to make the young man more familiar with some of the elementary principles.

OBITUARY

DEXTER DWIGHT MAYNE

DEXTER DWIGHT MAYNE, for twenty-six years principal of the School of Agriculture, University Farm, St. Paul, Minnesota, died at Gulfport, Mississippi, on Saturday, December 14, and was buried at Platteville, Wisconsin, on Friday, December 20.

Professor Mayne was born at Beetown, Wisconsin, May 14, 1863, the son of Nicholas and Mary (Treloar) Mayne. He was graduated from the State Normal School at Platteville in 1883. He was principal of schools at Fennimore, Wis., from 1883 to 1884; Elkhorn, 1884 to 1889; Fort Atkinson, 1889 to 1893; superintendent and principal of schools, Janesville, Wisconsin, 1893 to 1901, and superintendent of schools, Ishpeming, Michigan, 1901 to 1902. From the last-named place he went to the School of Agriculture of the University of Minnesota, a technical school giving a course of three years, of six months each year, and open to pupils who had had only grammar-school preparation, on the same campus but separate from the College of Agriculture of the University of Minnesota. In this position he continued until his death.

Under Professor Mayne the School of Agriculture of the University of Minnesota had a remarkable growth, the attendance at one time approaching or exceeding one thousand. Its success was so marked that it attracted attention throughout the country and led to the establishment of two similar schools, one at Crookston and one at Morris, Minnesota, within a few years after Professor Mayne assumed the headship of the school at University Farm. Three years ago a third school of the kind was opened at Grand Rapids. The establishment of these outlying schools, with the introduction of agriculture into the curriculums of high schools of Minnesota, led to a falling off in the attendance at the school at University Farm, but the school has continued throughout to attract large numbers of students interested in training themselves for leadership in farm and rural home life.

Professor Mayne was a man of alert mind and prompt and sure decisions, of persuasive friendliness and of resolute will. He was quick to decide and quick to act, and persistent in following through any project undertaken for the development of his students. He gave much time to intimate personal contact with the young men and young women under him, and his students were swift to respond. Their confidence, once won, never weakened. He had the extraordinary gift of imparting inspiration and

awakening ambition, and throughout the central northwest to-day are great numbers of young men and young women, graduates or former students of the school, in positions of leadership and influence. He was also held in highest regard by his colleagues in the university department of agriculture.

W. P. K.

RECENT DEATHS

DR. ASAPH HALL, of the U. S. Naval Observatory, who retired from a professorship of mathematics in the U. S. Navy on June 30 of last year, died on January 12 in his seventy-first year.

WILLIAM ALLEN ORTON, director of the Tropical Plant Research Foundation, died on January 7 at the age of fifty-two years. Dr Orton ended twenty-five years' service as plant pathologist of the Department of Agriculture in 1924.

DR. H. J. PACK, entomologist of the staff of the Utah Agricultural Experiment Station, died on January 5.

WILLIAM EDWARD MEEHAN, designer and for seventeen years director of the municipal aquarium in Fairmount Park, Philadelphia, died on January 2 at the age of seventy-six years. Mr. Meehan had been an associate editor of *The Public Ledger* for fifteen years and was the botanist of the Peary Relief Expedition to North Greenland in 1892.

HENRY J. COX, aged sixty-six, meteorologist of the U. S. Weather Bureau in Chicago, died on January 7.

DR. JOHN ROBERT BENTON, dean of the college of engineering in the University of Florida, has died at the age of fifty-three years.

COLONEL ROBERT A. MARR, inventor and civil engineer, died on January 2 at the age of seventy-three years. Colonel Marr was head of the department of civil engineering of the Virginia Military Institute and later became dean of engineering at the Virginia Polytechnic Institute.

PROFESSOR PHILIP JACOB WHITE, professor of zoology at the University College of North Wales, Bangor, died on December 26 at the age of sixty-seven years. He was director of the Puffin Island Biological Station. He reached the age limit for retirement two years ago, but was granted an extension till the close of the present session.

THE following resolution was adopted at the mid-winter meeting of the Western Society of Naturalists:

The members of the Western Society of Naturalists have learned of the tragic disaster to the ship *Carnegie* which interrupted important researches in terrestrial magnetism, meteorology and oceanography and caused the untimely death of Captain J. P. Ault, a man unique in the history of science, combining the highest scientific attainments with unusual qualities of leadership; be it, therefore,

Resolved, That the Western Society of Naturalists at their mid-winter meeting at the Hopkins Marine Station, at Pacific Grove, extend their sincere sympathy to Captain Ault's family and to the Carnegie Institution of Washington; and, be it further

Resolved, That copies of this resolution be sent to Mrs. Ault, to the Carnegie Institution of Washington and to SCIENCE.

G. F. MCEWEN,
T. G. THOMPSON,
E. G. MOBERG, *chairman*

SCIENTIFIC EVENTS

MUSEUM AND LABORATORY FOR THE STUDY OF INDIAN LIFE

PLANS for the construction of a museum and laboratory for the study of American Indian Life in the heart of the cliff-dwelling area of New Mexico have been completed with the selection as architect of John Gaw Meem, of Santa Fé, member of the American Institute of Architects, according to an announcement by the Board of Trustees of the Laboratory of Anthropology at Santa Fé. The first unit of the proposed group of buildings will be built in the early spring with funds granted by John D. Rockefeller, Jr.

Mr. Rockefeller has given $200,000 for the erection and equipment of the first of the ten units planned, and will also contribute the income of a fund of $300,000 toward the budget of the laboratory for five years. Additional contributions, and possibly a permanent endowment, are expected from the same source if the laboratory proves its value as a lasting operative institution.

When completed the project will provide research laboratories, museum exhibitions, libraries, lecture halls and adequate facilities for graduate instruction in archeology as well as public education in the history of America's native races. Standing in the center of the cliff-dwelling area, and near the largest Pueblo Indian villages, the laboratories will enable research workers to study New Mexico's valuable historic relics without their removal from the state.

The buildings will be erected on a fifty-acre plot on the outskirts of Santa Fé. Mr. Meem has designed the plans for the group in what is termed the "Santa Fé" style of architecture. The first unit will cover a ground area of about 8,000 square feet and will contain on one floor administrative offices, exhibition

halls, laboratories, studies, an assembly hall and lounge, and a library.

"The unit which will be built early this spring," the announcement of the trustees says, "is to be the first of a series of buildings which, it is hoped, will ultimately bring together records and exhibits of all the important aspects of research work in the Southwest. We intend to coordinate the studies of American Indian life from the earliest times to the present, to facilitate research work and to help spread a knowledge and understanding of America's archeological past."

A field training school will be maintained for the benefit of graduate students and residence scholarships made available for the assistance of advanced students.

Other units to be added to the central structure are the Navajo Building, to be constructed immediately, an Ethnological Laboratory, four laboratories for allied branches of research, an Administration Building, living quarters for the staff, a residence for the Director, a garage and "corrals," a central heating and power plant, and experimental gardens.

The Laboratory of Anthropology was incorporated in New York in 1927 and has been engaged in attaining its project since then. Its trustees include representatives of leading universities, museums and scientific institutions throughout the country. They are:

R. B. Dixon, Harvard University; Kenneth M. Chapman, Indian Arts Fund; Ralph Linton, Field Museum of Natural History; J. A. B. Scherer, of the Southwest Museum, Los Angeles; Earl H. Morris, University of Colorado; G. G. MacCurdy, Yale University Prehistoric School; N. C. Nelson, American Museum of Natural History, and F. G. Speck, University of Pennsylvania; A. V. Kidder, Phillips Academy, Andover, Mass.; Franz Boas, Columbia University; A. L. Kroeber, University of California; Byron Cummings, University of Arizona; E. L. Hewett, Museum of New Mexico; C. E. Guthe, University of Michigan; N. M. Judd, Smithsonian Institution; Sylvanus Morley, Carnegie Institution of Washington, and E. A. Hooton, Harvard University; F. W. Hodge, Museum of the American Indian; Clark Wissler, Museum of Natural History; H. C. Bumpus, American Association of Museums; F. C. Cole, University of Chicago; R. V. D. Magoffin, Archeological Institute of America; Jesse L. Nusbaum, Mesa Verde National Park, and Amelia Elizabeth White, Eastern Association on Indian Affairs; Elsie Clews Parsons, Southwest Society; Margaret McKittrick, New Mexico Association on Indian Affairs; Frederick Harvey, Fred Harvey Company; W. H. Simpson, Santa Fé Railroad; J. F. Zimmerman, University of New Mexico; Leslie Spier, University of Washington; Bronson M. Cutting, United States Senator from New Mexico.

Those who signed the original articles of incorporation were Kenneth M. Chapman, R. B. Dixon, F. W. Hodge, Neil M. Judd, A. V. Kidder, Sylvanus Griswold Morley, and Clark Wissler. The executive committee now includes A. V. Kidder, chairman; Kenneth M. Chapman, secretary; R. B. Dixon, F. C. Cole, Amelia Elizabeth White, and J. F. Zimmerman.

STORROW FELLOWSHIPS IN GEOLOGY AND GEOGRAPHY

IN view of the scarcity of fellowships in geology and geography as compared with other physical and chemical sciences, those interested in the former subjects will welcome the announcement by the chairman, Dr. Arthur Keith, of the division of geology and geography of the National Research Council, that through the generosity of Mrs. J. J. Storrow, of Boston, several liberally planned fellowships in geology and geography will be granted under that division of the council. The only condition imposed by the donor is the desirable provision that she will have opportunity to establish personal acquaintance with the fellows.

The purpose of the fellowships is to enable young men of unusual ability and aptitude for the work to embark in the field of research in any branch of geology or geography. They are without restriction as to institution, though the committee may designate the institution to which a given fellow shall go for given work. No advanced degree will be required, though it is presumed that the candidates will have had training equivalent, at least, to the bachelor's degree. Further, the fellow need not be a candidate for an advanced degree, the view of the committee being that training for research and the launching of gifted young men in successful research are more important than the routine attainment of a degree. The committee desires especially to aid several men of outstanding qualities who lack funds for the final training needed to carry them over under the best guidance into well-founded research careers. The awards will not be confined to students but may go to young men out of college who by means of a fellowship may be enabled either to enter on a research career for which their preparation is already nearly complete, or to secure further training in a line of research which they have already entered with evident promise of making good.

The annual stipend is $1,200 or more, depending on the conditions obtaining in the particular case. Commitments will be for one year only. Applications will be welcomed. Much care will be taken to determine the qualifications and particularly the ability of the candidate to make the best use of the opportunities to be offered. The applicant should state age, training, work done, objectives and plans, giving references. Correspondence should be addressed to Chairman, Committee on Fellowships,

Division of Geology and Geography, National Research Council, B & 21st Streets, Washington, D. C., and should be in the hands of the committee before April 5.

ECOLOGICAL MONOGRAPHS

ARRANGEMENTS have been completed between The Ecological Society of America and Duke University for the establishment of a new quarterly journal to be known as *Ecological Monographs* which will begin publication in January, 1931. The Ecological Society becomes responsible for appointing all but one of the editorial board, the other member will represent Duke University, and for the editorial policy of the new series, while Duke University assumes charge of publication and of financial aspects.

At the Des Moines meetings, the society elected:

A. S. Pearse, Duke University, Editor.
Barrington Moore, Editor of *Ecology*, ex-officio associate editor of *Ecological Monographs*.

The other associate editors elected are:

H. A. Gleason, Brooklyn Botanical Garden
J. E. Weaver, University of Nebraska
E. N. Transeau, Ohio State University
R. N. Chapman, University of Minnesota
R. E. Coker, University of North Carolina
Chancey Juday, University of Wisconsin

In view of Professor Pearse's absence in Japan until the opening of the academic year of 1930–31 papers intended for submission to the new journal should be sent forward to any one of the above editorial board or they may be sent forward to the undersigned, who as chairman of the society's committee on the publication of longer papers is still actively interested in this project.

Present plans call for the issuing of *Ecological Monographs* as a quarterly journal devoted to the publication of researches in longer papers of ecological interest from the entire field of biological science. It proposes to undertake the publication of papers from 25 to 100 printed pages in length which are too long for *Ecology*, the other journal established by the society. The board of editors will be glad to consider thoroughly scientific manuscripts which deal with any aspect of ecological investigation, including such practical fields as—horticulture, economic entomology and forestry, but will not include technical papers which deal solely with economic problems.

The subscription price will be six dollars per annual volume of about 500 pp. Sustaining members of the Ecological Society will receive both *Ecological Monographs* and *Ecology* without increase in dues, just as all regular members now receive the latter journal.

W. C. ALLEE
THE UNIVERSITY OF CHICAGO

CANADIAN PHYTOPATHOLOGICAL SOCIETY

THE first meeting of the Canadian Phytopathological Society was held in Ottawa on December 19 and 20, 1929. In addition to the presentation of some twenty papers on various phases of plant pathology, organization was completed and the following officers elected for the year 1930:

President—H. T. Güssow, Dominion botanist, Department of Agriculture, Ottawa, Canada.
Vice-president—W. P. Fraser, professor of biology, University of Saskatchewan, Saskatoon, Sask.
Secretary-Treasurer—T. G. Major, tobacco division, department of Agriculture, Ottawa, Canada.
Councillors—D. L. Bailey, professor of plant pathology, University of Toronto; J. G. Coulson, professor of plant pathology, McGill University.

The new society is an outgrowth of the former Canadian Division of the American Phytopathological Society formed in 1919 and which has now been dissolved. The society will be affiliated with the Canadian Society of Technical Agriculturists. Publication of the "Proceedings" will be commenced immediately, and arrangements are being completed for the publication of other papers in "Scientific Agriculture" until such time as the society is in a position to publish its own journal.

T. G. Major, secretary-treasurer, writes that the new society extends greetings to the phytopathologists of the United States, and bespeaks their cooperation.

OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE proceedings of the Des Moines meeting of the American Association for the Advancement of Science and the associated societies will be reported by the permanent secretary and printed in the issue of SCIENCE for February 7. As has already been noted here Professor T. H. Morgan, director of the Kerckhoff Laboratories of the Biological Sciences of the California Institute of Technology, was elected president of the association to succeed Dr. R. A. Millikan. Other officers elected at Des Moines are:

Vice-presidents and Chairmen of the Sections

A—Mathematics: G. A. Bliss, the University of Chicago
B—Physics: F. K. Richtmyer, Cornell University
C—Chemistry: James F. Norris, the Massachusetts Institute of Technology
D—Astronomy: D. W. Morehouse, Drake University
E—Geology and Geography: Edson S. Bastin, the University of Chicago

F—Zoological Sciences: W. A. Riley, the University of Minnesota
G—Botanical Sciences: E. J. Kraus, the University of Chicago
H—Anthropology: Carl E. Guthe, the University of Michigan
I—Psychology: Edwin G. Boring, Harvard University
M—Engineering: Frank B. Jewett, the Bell Telephone Laboratories
N—Medical Sciences: Louis B. Wilson, the Mayo Foundation
O—Agriculture: W. C. Coffey, the University of Minnesota
Q—Education: L. V. Koos, the University of Chicago

Members of the Council

Rodney H. True, the University of Pennsylvania
John C. Merriam, the Carnegie Institution of Washington

Members of the Executive Committee

Karl T. Compton, Princeton University
Robert A. Millikan, the California Institute of Technology

Committee on Grants

Arthur H. Compton, the University of Chicago

Board of Trustees of Science Service

Burton E. Livingston, the Johns Hopkins University

Finance Committee

A. S. Frissell, the Fifth Avenue Bank, New York

SCIENTIFIC NOTES AND NEWS

A BUST of Henri Poincaré, the great mathematician, will be placed in the hall of the Academy of Sciences, Paris.

IT is proposed, according to an Associated Press despatch from Paris, to confer on Mme. Curie the order of Commander of the Legion of Honor.

DR. WILHELM OSTWALD has been elected an honorary member of the Chemical Society, London.

PROFESSOR W. H. HOFFMANN, of the Finlay Institute, Habana, has been elected a member of the International Institute of African Languages and Cultures in London.

DR. ELMER A. SPERRY, at a meeting of the Engineering Association of Hawaii on December 13, was elected an honorary life member of that organization. Dr. Sperry was *en route* from Japan, where he had attended the World Engineering Congress, to the United States.

AN Associated Press dispatch announces that Orville Wright, "for the design and construction, with his brother, now deceased, of the first successful engine-propelled airplane," will be the recipient of the first Daniel Guggenheim Medal for notable achievement in the advancement of aeronautics. The award will be presented on the fiftieth anniversary of the American Society of Mechanical Engineers in Washington.

DR. LEE DE FOREST has been elected president of the Institute of Radio Engineers for 1930 to succeed Dr. A. Hoyt Taylor. Colonel A. G. Lee, radio engineer attached to the British Post Office in London, was elected vice-president. John V. L. Hogan and R. H. Marriott were reelected managers of the institute.

DR. HERBERT S. LANGFELD, professor of psychology and director of the laboratory at Princeton University, has been elected president of the American Psychological Association. Professor John F. Dashiell, of the University of North Carolina, and Dr. Arnold Gesell, professor of child hygiene and director of the psychological clinic at Yale University, have been elected directors.

ON account of the retirement of Dr. F. E. Smith, Professor F. J. M. Stratton, professor of astrophysics in the University of Cambridge, has been appointed acting general secretary of the British Association for the Advancement of Science for the period before the meeting of the association in Bristol in the autumn.

DR. R. L. KING, of the University of Pennsylvania, has been appointed associate professor of zoology in the State University of Iowa. Professor King will take up his work at the beginning of the second semester of the present school year.

WILLIAM EMBRY WRATHER, consulting geologist and petroleum engineer, of Dallas, Texas, has been appointed lecturer on the geology and economics of oil and gas at Yale University for the balance of the academic year.

DR. B. W. WEINBERGER has been appointed to the newly established chair of dental history and literature in New York University.

THE chair of geology in the University of Glasgow, recently vacated by Professor J. W. Gregory, has now been filled by the appointment of Mr. E. B. Bailey, of the Geological Survey.

PROFESSOR H. J. FLEURE, D.Sc., at present professor of geography and anthropology in the University College of Wales, has been appointed professor of geography in the University of Manchester.

PROFESSOR SYDNEY HOWARD VINES, who retired from the Sherardian chair of botany in the University

of Oxford in 1919, after serving for thirty-one years, celebrated his eightieth birthday on December 31.

THE title of emeritus professor has been conferred by the University of London on Dr. E. A. Gardner on his retirement from the Yates chair of archeology at University College, and on Dr. F. W. Oliver on his retirement from the Quain chair of botany at the college.

DR. LOUIS A. BAUER, director of the Department of Terrestrial Magnetism of the Carnegie Institution of Washington since the establishment of the department on April 1, 1904, retired from this position on January 1, 1930, with the title of director emeritus. Provision is also made whereby if his health permit he may carry on studies during the coming year as a research associate of the institution. Mr. John A. Fleming, associated with Dr. Bauer as chief assistant in the Department of Terrestrial Magnetism since 1904, as assistant director for observational and administrative work during 1922 and 1923, and as assistant director in charge of operations since 1924, continues in charge of the work of the department with the title of acting director.

PROFESSOR ROBERT ROBINSON, of the University of London, has succeeded the late Professor W. H. Perkin as Waynflete professor of chemistry at the University of Oxford.

THE promotion has been announced by the U. S. Forest Service of District Forester C. M. Granger, of the Pacific Northwest District, to the position of head forest economist in charge of the nation-wide Forest Survey now being initiated by the Forest Service. Certain phases of the survey, such as the forest resource inventory and the study of growth, will be carried out by the regional forest experiment station under the general direction of Mr. Granger.

DR. LUCIUS HERBERT MERRILL, professor of biological and agricultural chemistry at the University of Maine, has been granted leave of absence and will devote a large part of his time to the work of state geologist, a position to which he was appointed last year by Governor Gardner. Dr. Merrill is a member of the class of 1883 and has served the university for 42 years, first as chemist in the Agricultural Experiment Station, and later as head of the department of biological and agricultural chemistry in the College of Agriculture. He is a brother of the late Dr. George Perkins Merrill, former head curator of geology of the National Museum at Washington.

PROFESSOR EMANUEL FRITZ, of the forestry division, College of Agriculture, the University of California, has left for Cornell University where he will be during the spring session exchange professor of lumbering in the forest school. The exchange is with Professor A. B. Recknagel, who has just completed a semester at the University of California.

DR. LEWELLYS F. BARKER, professor emeritus of medicine of the Johns Hopkins University School of Medicine, who has been lecturing at the University of California, expects to return to Baltimore in February by way of Panama, where he will attend the second annual congress of the Pan-American Medical Association.

DR. JAMES G. NEEDHAM, professor of entomology and limnology at Cornell University, will have charge of the work in field zoology at West Virginia University for twelve weeks, beginning about June 7. Transportation will be by motor cars and camps will be established, for a week or more each, at various parts of the state. The work will be carried on in cooperation with the field courses in botany so that a student may take one or both subjects during the summer.

THOMAS ADAMS, general director of plans and surveys of the Regional Plan of New York and its environs, begins during the second half of the academic year a study for the new Harvard School of City Planning. He will conduct research on the optimum densities for residential distribution in English garden cities and suburbs. During the last part of the second half of the academic year Mr. Adams will give before the School of City Planning several lectures on the subject of his investigation.

DR. H. DE FOREST, professor of botany at the University of Southern California, will be on sabbatical leave during the second half of this year. He will devote some six months to an investigation of the vegetation of southern California.

PROFESSOR ERNEST E. JUST, head of the department of zoology of Howard University, sailed on January 2 for Berlin where he will spend six months as guest investigator in the Kaiser Wilhelm Institut für Biologie, Berlin-Dahlem. Dr. Just was recently elected to the vice-presidency of the American Association of Zoologists.

DR. JOSEPH H. BARACH, for nineteen years a member of the staff of the Presbyterian Hospital of Pittsburgh, has been appointed director of the Falk Clinic, a free dispensary where eight hundred persons may be treated daily. The clinic, now under construction, is to be part of the medical center of the University of Pittsburgh and has been established by a gift of $900,000 from Maurice Falk and the late Leon Falk and Leon Falk, Jr., business men of Pittsburgh. Plans for the building were drawn by E. P. Mellon, architect, assisted by W. L. Smith. Dr. Michael

Davis, a member of the staff of the Rosenwald Foundation of Chicago, was consultant. The clinic will be the clearing house for the entire hospital group. Every department represented in the hospital group will be included in the clinic.

PROFESSOR G. H. PARKER, of Harvard University, addressed the Mayo Foundation of Rochester, Minnesota, on January 2, 1930, on the "Secretory Action of the Nervous System."

THE Stanford University Medical School announces the forty-eighth course of popular medical lectures to be given on alternate Friday evenings, beginning on January 10. These are: "Diabetes, the Cause and Cure," Dr. Horace Gray; "Some Sanitary Sins of the Orient," Dr. Alfred C. Reed; "Dental Caries as Viewed by the Bacteriologist," Dr. T. D. Beckwith; "What Medicine has to Offer the Nervous Patient," Dr. Henry G. Mehrtens; "Lessons from the Biography of Genius," Dr. Lewis M. Terman, and "Poisonous Animals," Dr. Karl F. Meyer.

EDWARD STEPHEN HARKNESS, of New York, has given to Yale University funds for the construction of dormitories on the lines of the colleges of the English universities. Five new quadrangles will be built and three existing groups reconditioned with the funds. Under the house plan from 200 to 250 students will live together under the leadership of a member of the faculty. The gift, it is understood, also provides for an endowment fund to be used as salaries for residential heads and for the establishment of the tutorial system of teaching. The amount is reported to be larger than the $12,000,000 that Mr. Harkness recently gave to Harvard University for the same purpose.

HARVARD UNIVERSITY will receive an unrestricted bequest of nearly $5,000,000 under the will of the late Stuart Wyeth, of Philadelphia.

THE Commonwealth Fund will give this year and in 1931 and 1932 $35,600 to the Columbia Dental School to study the causes of dental disease. The study will be made in cooperation with a group at Yale University engaged in a similar project under a grant from the Rockefeller Foundation, and also with other research workers in this country and Europe.

PLANS for the development of the department of chemistry of the University of Chicago, made possible by the new George Herbert Jones Laboratory, were outlined by Professor Julius Stieglitz, chairman, at a dinner which concluded the recent dedication program. The laboratory, gift of the Chicago citizen whose name it bears, provides facilities for more than a hundred research workers in chemistry. The cost was $665,000. The Rockefeller Foundation and the General Education Board have contributed $300,000 in endowment for the development of the staff of the department and for the purchase and maintenance of equipment necessary for their research work. In addition, the Chemical Foundation appropriated the sum of $10,000 a year for five years for research in chemistry applied to medicine. Pierre S. du Pont has given $10,000 a year for two years, to be expended in research in cancer from the chemical end, under the direction of Associate Professor Morris Kharasch. Strengthening of the staff, in anticipation of the improved opportunities afforded by the new laboratory, has already been achieved. Dr. Kharasch, of the University of Maryland, was brought to the University of Chicago a year ago. On January 1 Professor T. R. Hogness, now of the University of California, joined the staff as a permanent member. He will enable the department to develop more rapidly and intensively the application of light to the solution of chemical problems, especially those in connection with molecules and atoms. Dr. Preston Harris, at present of the department of physics, will join the staff in the spring quarter. His field of research is the application to chemical problems of the X-ray.

THE fourteenth annual meeting of the Pacific Division of the American Association for the Advancement of Science will be held at the University of Oregon, Eugene, June 18–21, 1930. It is five years since a meeting of the association has been held in the Pacific Northwest, the last having been held at Portland in 1925. An attendance of approximately five hundred representatives of many of the associated societies and of several groups not formally associated is expected. The dormitories of the university will accommodate about four hundred. According to preliminary plans, a symposium on trees will be one feature of the program. Members proposing to present papers are reminded that titles must be sent in to secretaries of societies not later than May 1. Preliminary announcements will be sent out in the middle of March.

THROUGH its applied mechanics division, The American Society of Mechanical Engineers will participate in the Third International Congress of Applied Mechanics to be held in Stockholm from August 24 to 29. The division has appointed a special committee, of which Dr. S. Timoshenko is chairman, to take care of the details, in connection with its part in this congress. The field of interest of this coming convention lies in hydrodynamics and aerodynamics, theory of elasticity,

and rational mechanics. The applied mechanics division of the society is also arranging a tour of the most important European laboratories in mechanics. Following the Stockholm Congress there will be a congress on general mechanical engineering to be held in Liège, Belgium, from August 31 to September 7, which is being held at the same time as the Belgium International Exposition.

THE tenth summer term of ten weeks of the American School of Prehistoric Research, under the direction of Professor George Grant MacCurdy, of Yale University, assisted by Mr. J. Townsend Russell, Jr., of the U. S. National Museum, will open in Paris on July 1. The field to be covered includes Paris museums, Valley of the Soame at Amiens, Brittany, northern Spain with excavations near Santander, the Pyrenees, Dordogne with excavations at St. Leon-sur-Vézère and Switzerland. In addition, the students will have opportunities to do field work in Czechoslovakia under the supervision of Messrs. V. J. Fewkes, of the University of Pennsylvania, and Robert W. Erich, of Harvard University. Applications for enrollment should be made as soon as possible to Dr. George Grant MacCurdy, Peabody Museum, New Haven, Connecticut.

AFTER a stay of four months the Oxford University Expedition to British Guiana has decided to return to England. It is stated that the expedition, which is under the leadership of Major R. W. G. Hingston, has secured what is said to be the best botanical and bird collections yet made in the forests of British Guiana.

Industrial and Engineering Chemistry reports that a chemical research bureau, with American participation, has been established in Zurich, Switzerland, under the name of the Colloid Chemical Research, Inc. The purpose is not only to conduct chemical research, particularly in the colloid chemical field, but also to acquire patents and processes for commercial development and to erect laboratories in Switzerland and elsewhere.

THE U. S. Civil Service Commission announces examinations for associate and assistant agronomist, applications to be on file by February 12. The examinations are to fill vacancies occurring in the Bureau of Plant Industry, Department of Agriculture, for duty in the field. At present there is a vacancy in the associate grade at Bellingham, Washington, and there are vacancies in the assistant grade at Huntley, Montana, and Fort Collins, Colorado. The entrance salaries range from $3,200 to $3,700 a year for the associate grade and from $2,600 to $3,100 a year for the assistant grade.

DISCUSSION

THE MICROMETRIC MUDDLE

IN an article with this title published in a recent issue of SCIENCE,[1] Mr. John P. Camp discusses the use and the interpretation of the symbol μμ. One might infer from the penultimate sentence of his article that he thinks that such questions might properly be settled by the simple process of counting the noses of those on the two sides. However such a method may work in politics, it scarcely accords with scientific ideals and, if adopted, would lead to ever-increasing disorder and confusion. It is a matter of common experience that new terms and new uses of old ones are most enthusiastically proposed and most avidly seized upon by those least qualified to decide what is needed and what accords best with established custom.

The confusion to which he refers seems to have arisen from a very common, but incorrect, definition of the micron (μ). It is commonly defined as a thousandth of a millimeter, and this suggests that μ indicates the one thousandth part. Neither is correct. The length of a micron is equal to the one thousandth part of a millimeter, but the proper definition of the micron is the millionth part of a meter. In the metric system, all units of the same kind refer to the fundamental unit, in this case the meter. As we have the myriameter (10,000 m), the kilometer (1000 m), the hectometer (100 m), the dekameter (10 m), the decimeter (0.1 m), the centimeter (0.01 m), the millimeter (0.001 m), so we have the micro-meter—called the micron—(0.000,000,1 m). With but few exceptions, the symbol of each unit is the dyad formed from the initial letters of the prefix and the name of the fundamental unit. According to this rule, the symbol for the micron would be μm, but μ has been used universally, I believe. Not only here, but in every case, in the metric system the prefix micro- and the symbol μ denote the one millionth part; for example, microsecond, microgram, microfarad, micromicrofarad, microhenry, microhm, microlambert. Consequently, consistency demands that μμ shall indicate the millionth of a millionth, and nothing else, and that mμ shall indicate the thousandth of a millionth. Furthermore, both must refer to the basic unit, not to a subsidiary one.

The unfortunate use of μμ to indicate a thousandth part of a micron seems to have been introduced by Kayser in 1883. It is well sponsored and probably

[1] SCIENCE, 70: 453, November 8, 1929.

arose in the manner already indicated, but it violates two principles of the metric system: (1) The placing of a subsidiary unit on the same plane as its primary; (2) the introduction of two symbols to indicate the same thing. For these reasons its use should be discontinued.

It is difficult to see how the use of the correct symbols can lead to any misunderstanding, as they fit into a well-known and orderly scheme. Even if one suspected that a writer had used μμ incorrectly, the context would usually show unambiguously what he meant, and if it did not, then the writer should be given the benefit of the doubt and be made to bear the burden of any error so resulting. The only difficulty in discarding the incorrect use of μμ arises from the human disinclination to break a bad habit.

Mr. Camp seems to imply that the Bureau of Standards is primarily responsible for the use of mμ to indicate the thousandth part of a micron. Were that true, it would be one more reason for the citizens of this country to be proud of the bureau, for it is a move in the direction of order and simplicity. Sixteen years ago, Ch. Ed. Guillaume, who is the director of the International Bureau of Weights and Measures, stated,[2] "La notation μμ, souvent employée, est défectueuse et doit être abandonnée," and gave mμ as the proper symbol for the thousandth part of the micron.

To conclude: In the symbolism of the metric system, the prefix μ denotes the one millionth part, and the prefix m denotes the one thousandth part; a micron (μ) is the one millionth part of a meter and, consequently, is equal to 0.001 mm; a thousandth part of a micron is called a millimicron, it is the one thousandth part of a millionth of a meter, its symbol is mμ; a millionth part of a micron is called a micromicron, it is the millionth part of a millionth of a meter, its symbol is μμ. Any departure from this is a violation of principles identified with the metric system and is to be deprecated as leading to disorder and confusion.

N. ERNEST DORSEY

NATIONAL RESEARCH COUNCIL,
DECEMBER 3, 1929

THE EFFECTIVENESS OF A PLANT QUARANTINE

THE actual degree of effectiveness of plant quarantines in preventing the spread of insects is not known. Proponents of these quarantines will maintain that the presumption of effectiveness should be allowed them. The fact that the Mediterranean fruit-fly has not yet appeared in California will be ascribed by them solely to the vigilance of quarantine officers. The fly is not here. There has been a quarantine against it. Consequently the absence of the fly is to be credited to that quarantine. Opponents of the quarantines may doubt—or even sneer—as much as they please, but they can prove nothing.

Undoubtedly there are many peculiar factors entering into and influencing the spread of insects, even under natural conditions. Why, for example, should a single, isolated plant—as has been observed by the writer in field collecting—be heavily infested with a certain scale insect, while whole thickets of the same plant less than a hundred feet away have none of it at all? Why has the Mediterranean fruit-fly been so unexpectedly considerate as to confine its efforts in Florida purely to cultivated fruits, as is reported to be the case? We may agree whole-heartedly with a recent investigating committee which has remarked, "That infestations have not been found in adjoining states where much fruit was shipped previous to the discovery of the infestation [in Florida] is difficult to explain."[1]

One example which affords a clean-cut test of this matter is at hand. A mealybug (*Pseudococcus brevipes* Ckll.) occurs abundantly upon pineapples in the Hawaiian Islands. This insect is a general feeder and is established on various hosts in Florida, where it is quite common, and also in Texas, but is not known to occur in California. There is no reason to suppose that it will not live in at least some part of this state and upon some plants that are grown here, but although I have been identifying mealybugs for various county commissioners of horticulture for several years, it has never come to me, and it may be assumed that it is not in the state.

This insect is not supposed to pass the quarantine barriers and it is one of the insects that have commonly been reported as among the "pests intercepted" by the quarantine officials. The presumption would therefore be that its absence from California is due to the vigilance of these officials.

Yet I have several times taken this insect alive on bananas and pineapples in markets in this state. In order to check up once more I looked for it in a market a few days ago. The one pineapple in this market had several living specimens upon it.

The facts of the matter, then, are that this insect must have come into California alive many thousands of times since the quarantines were instituted. Yet it is not established in the state. Whatever credit the quarantine officials may assume to themselves for the exclusion of other pernicious species, they can not lay claim to such credit in this case. The presumption of their effectiveness can not be maintained. The

[2] "Recueil de Constantes Physiques," page 5, Gauthier-Villars, Paris, 1913.

[1] Official Record, U. S. Dept. Agriculture, Vol. 8, No. 46, p. 8. November 14, 1929.

reasons for the failure of the insect to establish itself here are biological.

With definite examples of this sort before us, those of us who are unconvinced of the value of the plant quarantines—except as a substitute for a tariff—may be pardoned a certain amount of reluctance to concede that presumption of effectiveness to which the supporters of these measures are prone to appeal. We may with equal justice claim that in all probability there are other cases where biological factors and not the quarantine officers have been the effective barrier.

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THE DISTRIBUTION OF PAUROPUS

PAUROPUS HUXLEYI, a minute centipede-like animal with nine pairs of legs in the adult, was described by Sir John Lubbock. Specimens were found in England in 1866. Since then they have been reported from the continent of Europe, in the eastern states of this country and many other places. Actual records are from Sweden, Denmark, Germany, Austria, Italy, Chile, Paraguay, Argentina, Australia and Siam.

It was not until November, 1927, that I found them in California. Earlier records than this were of specimens in the New England region, Long Island, near Philadelphia and Indiana. The last record that I have found was by S. R. Williams and R. A. Hefner from Ohio.

In the summer of 1928 I found them in southern New York state. Since then I have collected them from a number of places. My first records were from southern California not far from Claremont in the college park under the live-oak trees, but I also found them in abundance in my own back yard or about a mile from the first location. I also found them near Laguna Beach in Orange County and in several situations in the San Gabriel Mountains, including one place among the pines at seven thousand feet altitude.

Two places in Mexico furnished specimens, although many other places were searched. One of these was from Lower California about a hundred miles below the border. Another lot was found not far from Mexico City. Several places were searched in Cuba without success. Neither were they found in Florida or other southern states, but in these last, at least, conditions were not favorable. Ideal places were found in many parts of California, Washington, Oregon, Utah, Montana, Wyoming and Idaho, but no specimens. However, several were collected in the deep coniferous forests on the slope of Mount Hood, Oregon, and some were found on Catalina and Santa Cruz Islands off the southern California coast.

Often, under certain conditions, it is not possible to find them in given localities, even though they are known to occur. If it is very wet or very dry they may not be seen, however abundant they may be at other times. We have not found them easily by means of funnels or sieves, and this may account for their apparent infrequency. The under-sides of stones or logs slightly dampened by recent rains seem to be the best situation to see them. Here they may be found among the more numerous small white Collembola.

After studying a considerable number of specimens from one place and comparing them with others of distant regions I at first came to the conclusion that there were about as many differences between individuals in one place as between specimens from widely separated localities, but more detailed study has convinced me that at least two distinct forms are in my collection. These, according to the descriptions of Hansen and others, are distinct genera. There is, then, a good chance of a number of species in my collection.

They are difficult to study in detail as they are neither small enough nor large enough for the usual methods to be employed in determining the distribution of the setae, proportions of the joints or other characters which aid in distinguishing one species from another. These that I have at present belong to the genera Pauropus and Stylopauropus.

I wish to have further material, and I should be glad if any collectors who may find specimens of these interesting forms will communicate with me as soon as possible.

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AN UNBOUND TEXT AND NOTE BOOK

THE writer has been trying an experiment which has worked out so satisfactorily in one direction for him that it has seemed others might be interested in trying the same thing, if they have not already done so.

At the time of publication of a recently issued book the publishers were requested to furnish one unbound copy with holes punched at appropriate places on the left hand margin. The intention was to use this copy in loose-leaf form for work in the classroom in a course involving material covered by the book. In such a case the primary advantage of a loose-leaf book lies in the fact that one can readily insert blank pages for notes concerning corrections, new material or anything else of value for the work in hand. In fields of science experiencing rapid developments and changes this seems particularly desirable. Such an

arrangement makes of the book a combination text and note book for the instructor. A second advantage, which is of much less significance, lies in the fact that the loose-leaf type of book will open easily and lie flat on the instructor's desk, if a suitable binder is used.

As no commercial binder could be found having capacity for a sufficient number of pages, a home-made one was devised. Three half-round book rings (preferably with a hinge at either end of the flat section and the opening in the middle of the circular section) were soldered along the flat section to one side of a piece of brass (twenty-two gauge) at a distance apart to correspond with the holes in the sheets of the book. The piece of brass was slightly longer than the sheets and slightly wider than the length of the flat section of the rings. A two-piece folding back was made by cutting down ordinary folders of heavy press board designed for holding laboratory reports. In order to hold the folding backs in place a piece of sheet aluminum of the same size as the sheet brass was placed on top of them directly over the piece of brass, and the whole thing fastened together by means of round head, split paper fasteners put through holes made in both the pieces of metal and the press board cover.

Although such unbound copies of books are ordinarily unavailable, it seems probable that publishers would be willing to supply them if there was a demand, and also that suitable loose-leaf binders would be relatively easy to produce. The page proof, having every second page blank for notations, would be even better than the regular sheets if it was printed on the same grade of paper as the book. Its only disadvantage is the increased thickness.

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PEDOLOGY OR CHTHONOLOGY?

IN Dr. P. E. Brown's interesting discourse on "The New Soil Science," which appeared in the December 27, 1929, issue of SCIENCE, "pedology" is given as a synonym of soil science. Dr. Brown says: "Soil science or pedology, as it is now coming to be called, is not new."

Do Dr. Brown and other soil scientists who use the term "pedology" as a designation for their specialty know that this word has been employed for thirty-three years as the name of the science of the child, or child-study in its broadest sense? The term, which is also spelled paedology or paidology, was probably introduced by O. Chrisman in 1896.[1]

If soil specialists are in need of a name for their science and wish to conform to the criteria of etymologic excellence, why do not they choose the term *chthonology?* The word chthonography, a descriptive treatise upon soils, is now in limited use. If they should fear cacoepy in the use of the proposed neonym, they might give thought to adopting the term *humology,* which would no doubt be acceptable to lexicographers.

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SCIENTIFIC BOOKS

Icones Farlowianae, Illustrations of the Larger Fungi of Eastern North America. By WILLIAM GIBSON FARLOW. Farlow Herbarium, 20 Divinity Avenue, Cambridge, Massachusetts. 1929. 103 colored pls. $40.00.

THE mycologists of the old world have, during the last two centuries, supplied students of agarics, in rather numerous Icones and other publications, with an abundance of colored figures of these fascinating plants. With the exception of the Icones of Boudier, where the scientific training of the mycologist was to a remarkable degree linked with the talent of a real artist in the same man, no illustrations of mushrooms have appeared which remotely approach the beauty and scientific accuracy of the plates in the volume before us. It is, furthermore, the first extensive collection of colored plates of American agarics to be conceived, executed and published in this country.

As explained in the Introduction by Dr. E. A. Burt, who edited the work and wrote the accompanying descriptions, all the 103 plates included in this volume were completed as well as printed by 1908. The author employed two successive artists. The larger number of paintings included in the published volume were made by Mr. Joseph Bridgham; later, from 1902 to 1911, the author employed the well-known mycological artist, Mr. L. C. C. Krieger. The eminently successful reproduction of the colored drawings by the Boston Heliotype Printing Company enhances the value of the finished plates in a large measure. For the writing of the text and the final touches in the editing of the work, mycologists have to thank Dr. E. A. Burt, whose loyalty to his departed friend and teacher made of the difficult task a labor of love.

Dr. Farlow, fortunately, wrote the preface himself, an *apologia,* characteristic of the man. "The aim of

[1] See *Am. J. Psychol.,* 12: 268.

the author," he writes, "has been to furnish to those who are not in the possession of large libraries and collections the means of identifying the more striking and characteristic of our larger fungi," and, on the other hand, he hopes that such accurate presentations of endemic American species and of species which also occur in Europe may keep the scientific man from making erroneous identifications of these fungi or confusing their nomenclature.

The author has shown his usual acuteness in the matter of the forms selected. No one genus of agarics is given undue prominence; but that sense of proportion, so often lacking in teachers, by which we pass from the special to the general, from the known to the unknown, is beautifully exemplified in the selections made. Only three examples of the deadly Amanita group are included: *A. verna, A. solitaria* and *A. porphyria.* A plate of the typical "parasol mushroom" will impress that species on any beginner. Similarly, other plates depict strikingly such plants as *Cantherellus floccosus, Agaricus abruptibulbus, Cortinarius mucifluus, Cortinarius squamulosus, Claudopus nidulans* and other such common species.

Twenty-one species of Boletus are depicted: *B. sublutens, B. elegans, B. americanus, B. punctipes, B. granulatus, B. badius, B. dichorus, B. auriporus, B. piperatus, B. rubinellus, B. Ravenellii, B. subtomentosus, B. Russulii, B. speciosus, B. ornatipes, B. eximius, B. luridus erythropus, B. Frostii, B. chromapes, B. cyanescens* and *B. castaneus.* This list, with the exception of the rare *B. rubinellus* Pk., can be duplicated in the wooded northern portion of the United States and in Canada, as far west at least as the Rocky Mountains, and although the plants from which the illustrations were made were doubtless collected in northern New England, they represent a large proportion of the Boletus flora in the country mentioned and the favored possessor of the "Icones" can identify most of his findings with ease. Here, again, Dr. Farlow's experience in the field made him realize that in this group in which descriptions alone leave much to be desired, an illustration is above all essential. The individual species of this genus are very variable in appearance from the effects of weather conditions during their development, and the selection of such typical specimens as here represented is most happy. The autonomy of *B. punctipes* Pk. has been much questioned, but there is no doubt that it is a good species, and although it is oftener paler when fresh than here shown, the figures are quite convincing.

Among the agarics figured, thirty-nine are marked edible. With the exception of *B. luridus,* and possibly *B. rubinellus* and *B. Frostii,* the Boleti shown have nothing against them, although Dr. Burt refrains from marking some of them as to edibility.

A very limited number of species belonging to genera where the plants are small or minute have been included. Thus, there are only five species of Inocybe shown, one plate with two species of Leptonia, while the genera Mycena, Galera and Psathyra are not represented. In view of the fact that no microscopic characters are shown on these plates, it would have been very unwise to include species of these genera. During the last fifteen years or more, the study of the microscopic details found present and known to be of taxonomic value for the species of these genera has brought about a temporary uncertainty in the taxonomic placement or identity of the older species. The few species that are depicted can be recognized fairly well by external characteristics.

In the genus Naucoria there are two old European species, *N. Christinae* and *N. hilaris,* both of which are rare, except in local areas, and both of which have somewhat the aspect of *Hygrophorus conicus.* The fine plate of *N. Christinae* in the "Icones" should help in determining its geographical distribution. In thirty years of collecting the present writer has failed to locate any of this species, but has collected its nearest relative in the Adirondack Mountains. The latter grows on rotten wood.

Five newly named and described species are illustrated: *Inocybe amarescens* Farlow and Burt, *Lepiota brunnea* Farlow and Burt, *Tricholoma oliveum* Farlow and Burt, *Stropharia rugoso-annulata* Farlow and *Stropharia subcaperata* Farlow and Burt. The last species is, unfortunately, well described in the American literature as *Stropharia depilata* Fr. No hint is given in the text of the "Icones" that this American plant does not correspond to that of Europe.

Among the species of agarics rather rarely seen throughout this country and which are perfectly figured in the "Icones" may be mentioned: *Armillaria robusta* Fr., *Armillaria ponderosa* Pk., *Tricholoma decorosum* Pk., *Crepidotus dorsalis* Pk., *Entoloma cuspidatum* Pk. and *Gomphideus viscidus* var. *testaceus* Fr. Finally, near the end of the volume, several examples of Gasteromycetes, Polypori and Ascomycetes are to be found.

The intrinsic value of the book is to be found in the fine judgment displayed in the selection of material, in the infinite care taken in planning the details and in the ability to employ, without taking count of the cost, men who could put on paper that which nature had wrought and retain its purity of beauty and at the same time its scientific meaning.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

THE EFFECT OF ULTRA-VIOLET IN PRODUCING FUSION OF EGGS OF CHAETOPTERUS

DURING the summer of 1926 at the Marine Biological Laboratory, Woods Hole, Massachusetts, while repeating some experiments on the effects of ultra-violet radiation on eggs of *Chaetopterus* before insemination, in order to preserve series of them for cytological study, I noticed during the cleavage stages a tendency of the eggs to fuse. Such fused eggs developed as far as the trochophore stage, as many as fifteen eggs frequently taking part in the fusion. This result is best obtained by an exposure of sixty to seventy seconds, the eggs being 21.5 cm distant from a Cooper-Hewitt mercury vapor arc lamp. Since the effect of ultra-violet radiation at lower exposures, namely, thirty, forty and fifty seconds, produces profound alterations of the cortex, as I shall show in a forthcoming paper, I assume that the fusion due to longer exposures is likewise attributable to changes in the cortex. For example, in observations made on eggs inseminated after radiation, on August 12, I found the next day innumerable fused eggs. Some fused masses were made up of twelve to fifteen eggs. Subsequently, it was learned that fusion is more readily brought about in dishes containing closely crowded eggs, and in dishes with few eggs in which the eggs are brought into close proximity. The fusion therefore, is primarily the result of radiation, and not of overcrowding, since of two equal lots of eggs from the same female—one lot inseminated with and one without previous exposure to the ultra-violet rays and suspended in equal volumes of sea-water—fusion takes place only in the exposed lot. Eggs centrifuged before insemination, that show while living a gray cap which after fixation with solutions containing osmic acid proves to be a disk of oil drops, behave in the same way.

Ultra-violet radiation has another interesting effect on eggs of *Chaetopterus*. In the swimming stage, the single trochophores show the apical tuft of long cilia displaced sometimes as much as 90°. In the majority of cases, however, this displacement amounts to about 75°. This result seems to indicate a change in the original organization of the egg. While it is true that ultra-violet radiation induces some eggs to differentiate without cleavage, the types here described show fairly normal cleavage, especially after the shorter exposures.

I might point out also that at times normal fertilized eggs of *Chaetopterus* used during these observations failed to form a yolk lobe. This is invariably a sign that the eggs are not in optimum condition.

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A SIMPLE METHOD FOR EXPERIMENTAL PARTHENOGENESIS

HYPERTONIC sea-water is undoubtedly the simplest method for the experimental initiation of development. Hypertonicity is brought about by the addition of concentrated salt solution to the sea-water. The salts most commonly used are sodium, potassium and magnesium chloride. In practice, one makes up a 2.5 M solution of the first or second, or a 1.25 M solution of the third, varying proportions of the molar solution and sea-water being employed.

A very simple method for experimental parthenogenesis is as follows. To clean filtered sea-water, crystals of Na_2SO_4 are added to excess. After the supernatant sea-water has become perfectly clear, it is decanted. Eggs exposed to this sea-water plus Na_2SO_4 for thirty to sixty minutes show a high per cent. of cleavage and plutei on return to normal sea-water. While in the solution, the eggs show separated membranes. A nicer method is to take the supernatant sea-water after the addition of the Na_2SO_4 in excess and add it to sea-water in varying proportions. For example, one may set up dishes in a series, the first of which contains the Na_2SO_4 sea-water solution alone; the second, nine parts of the solution plus one part of normal sea-water; the third, eight and two parts respectively; the fourth seven and three, etc. To each dish equal amounts of eggs from the same female are added. Eggs are then transferred at fifteen-minute intervals during a period of two hours. In this way one may take into account any variations in response of eggs during a given season to exposure to hypertonic sea-water.

E. E. JUST

THE AMOUNT OF OSMIC ACID IN FIXING SOLUTIONS NECESSARY TO BLACKEN FAT

FIXING agents containing osmic acid are generally conceded to be the best for cytoplasmic fixations. There are several difficulties that militate against successful results with such agents. Preparations which the worker oftentimes describes as "over-osmicated" stain very poorly with Heidenhain's iron hematoxylin. Since this stain is so generally employed by cytologists fixatives containing osmic acid are less frequently used than others. Bouin's, for example, is many times preferred. Some of the difficulties in the way of successful results with osmic acid in combination with

other chemicals may be removed. One of the chief obstacles to success is the amount of osmic acid employed.

In the first place, most of the osmic acid on the market is of inferior grade and, what is perhaps just as important, not always of the weight labeled. Best results are obtained with the best quality of the acid. If a given tube supposed to contain one gram of osmic acid actually contains more, then what the worker makes up as a 2 per cent. solution is obviously of greater strength. Working with some of these inferior grades of osmic acid, tubes of which frequently contain more than one gram, I have found that I could get very satisfactory results by using as little as 0.75 cc of osmic acid in Fleming or Meves solution. Indeed, in some cases, the amount of acid used was less than half this amount. The cytoplasm is very well fixed, and the staining with iron hematoxylin leaves little to be desired. Such small amounts of osmic acid have been successfully employed in the fixation of most diverse types of cells. The criterion for the amount of osmic acid necessary for good cytoplasmic fixations is that which will blacken the oil drops in the centrifuged uninseminated egg of *Arbacia*. One need simply to prepare a solution of 1 gram of the acid in 50 cc of distilled water, then take 4 or 3.5 cc of this with chromic and acetic acids as used in Fleming or Meves solution, and prepare also other solutions using instead 2, 1.5 and 0.75 cc of osmic acid. Eggs after centrifuging are placed in these solutions for thirty to sixty minutes, after which they are washed in several changes of tap or distilled water and examined under the microscope. If the disk of oil drops, the so-called gray cap in the living egg, is blackened in that solution containing the least amount of osmic acid, this constitutes sufficient evidence that enough of the acid has been used. After the use of these solutions containing small amount of osmic acid, the cells are perfectly preserved, both as regards nuclear and cytoplasmic structures. After iron hematoxylin, the cytoplasm is a clear pale blue, mitochondria are stained dark blue and the chromosomes after breakdown of the nucleus are stained black. Results with this method are infinitely superior to those obtained by the so-called weaker Fleming. That is to say, in my experience at least, it is better to alter the amount of osmic acid alone, leaving the chromic and acetic acids in the proportions originally given by Fleming and Meves.

E. E. JUST

SPECIAL ARTICLES

VARIATION IN THE PERCENTAGE OF PROTEIN IN THE GRAIN OF A SINGLE WHEAT PLANT

THAT a plant may vary markedly in composition is common knowledge to all engaged in plant investigations. Causes and significance of such variations, particularly of the percentage of given proteins in wheat, have been the subject of much study, because of the relation this property of grain has to the quality of bread. As information was desired on the probable range of variation in the percentages of protein in the grain of single wheat plants, experiments were performed that were designed to obtain marked differences in this character of wheat. Some of the data which were obtained are given in the following table. The values given are those of the percentages of protein in the grain of different heads on the same plant. They were chosen as representatives of the lowest and the highest protein grain grown on an individual plant, but are not to be considered as the lowest or highest values that possibly could have been obtained.

Variation in the percentage of protein in wheat is directly related to that of the supply of nitrogen available to the plants at different growth periods—the later in growth a given supply is absorbed the higher the protein content of the grain. Thus an essential feature of the experiment was that of providing conditions whereby a given supply of nitrogen would become available late in the growing period of some stalks and early in case of others. This required that each plant have two distinct crops of stalks, one that arose early in its life and the other late. The requirement was obtained by planting seed in soil deficient in nitrogen in order to restrict stalk formation of the early growth period to one culm per plant. But later in growth—ninety days after planting—an ap-

TABLE I

VARIATION IN THE PERCENTAGE OF PROTEIN IN THE GRAIN OF DIFFERENT STALKS OF THE SAME WHEAT PLANT

Variety	Percentage of protein		Difference	
	Low	High	Actual	Percentage
Bunyip	13.6	17.6	4.0	30
Cedar	12.3	18.6	6.3	57
Dart's Imperial	10.9	11.1	.2	2
Early Baart	10.4	12.2	1.8	17
Fulcaster	8.2	11.4	3.2	39
Hard Federation	11.8	17.3	6.5	55
Sonora	6.4	14.0	7.6	119
White Australian	10.2	13.0	2.8	27

plication of 250 mgs of nitrogen per plant was added to the soil in the form of $NaNO_3$ in order to induce new stalk formation. Soon after this treatment tillers arose on various plants and thus two distinct crops of stalks, each capable of bearing grain, were produced. Because of the differences in time of the inception of these two crops of stalks on a plant, marked differences were obtained in their ripening. Usually, but not invariably, the grain of the parent stalk ripened before that of the tillers. As the amount of nitrogen supplied to each plant was more than the parent stalk could absorb but less than that which the tillers could utilize, the required conditions were met, namely, that of providing an ample supply of nitrogen during the later growth period of some stalks of the plant and early in case of others. The former produced high protein grain, the latter low protein grain.

The length of the interval between the ripening of the grain of two stalks on a plant or that of different plants grown under similar conditions appears to be of considerable importance and related to variation in the protein content of wheat. The correlation appears to be: the larger this interval, the larger the differences in the protein content of the grain. From these circumstances it follows that uniform ripening of all heads is an essential condition for the production of wheat of low variability. Inspection of the datum shows that a difference of one day or less in the ripening of two heads of wheat of some varieties can be detected in the composition of the grain. For example, the differences between the high and the low values of Cedar, Hard Federation and Sonora are 6.3, 6.5 and 7.6, respectively, which is equivalent to the accumulated change (difference) of 0.1 per cent. per day of the indicated amounts for 63, 65 and 76 days, respectively. But as the difference in time of ripening of the two crops of Cedar was only 36 days, the total difference of 6.3 is equivalent to a daily change (difference) of .17 per cent., a figure far above that of the experimental error in the method commonly employed for the determination of protein in wheat grain.

That a difference of one day in the ripening of two heads of wheat, or even of a few hours as happened to be the case in some varieties, can be detected in the composition of the grain appears at first consideration inconsistent with the general observation of the relationship between these circumstances. Mere change in the rate of desiccation or hardening obviously can not affect the ultimate percentage that a given constituent thereof has to the whole. As the protein content of wheat grain resolves itself into a relationship between two variable factors: (*a*) the amount of nitrogen available for grain, (*b*) the amount of grain in which a given quantity of nitrogen will be stored, so the correlation found in the variations in time of ripening of wheat with that of the protein content of the grain indicates that the time between final ripening and the period when the causes for variation can be operative is a fixed interval. In the case at hand the relationship is explained that the supply of nitrogen available for absorption was less than the potential quantity which would have been absorbed were it present, and the percentages of nitrogen found in two different heads of wheat indicate the quantities which are proportional to the proximity to harvest that comparable rates of the absorption of nitrogen were maintained.

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GLUTATHIONE IN PLANT TISSUES

GLUTATHIONE, because of its apparent function in the fundamental process of respiration, has become a substance of great interest. In 1927 Fink[1] published a micro method for the determination of glutathione in insect tissues. This method has been used by the writer in an attempt to determine the distribution of the dipeptide in plant tissue.

In making the determinations thin free-hand sections of the tissues were made. Immediately a solution of hot dilute acetic acid (approximately 20 per cent.) was poured over these. After a few seconds the acid was drained off and the sections were covered with 5 cc of a saturated solution of ammonium sulphate, $(NH_4)_2SO_4$, and 6 to 10 drops of a 5 per cent. solution of sodium nitroprusside, $Na_2Fe(CN)_5(NO)\cdot 2H_2O$. The tissue was allowed to stand for at least twenty minutes to insure the penetration of this solution. The section was then removed with enough of the solution to keep it moist. Several drops of dilute ammonium hydroxide (one part in three parts of water) were added. On the addition of the ammonium hydroxide a color, pale pink to a vivid purplish-red, flashed up through the tissues. This color lasted for a few seconds only, although longer in some tissues than in others. The difference in the intensity of the color is thought to be due to the difference in the amount of reduced glutathione present in the tissues.

Representatives of the different divisions of the plant kingdom were tested. The following examples are typical of the results obtained.

(*a*) *Thallophytes.*—Both Fungi and Algae were used. In sections of fresh sporophores of *Coprinus*, vivid color showed in the lamellae and extended gradually through the stipe. In *Rhozipus nigricans* the

[1] Fink, SCIENCE, 65: 143, 1927.

young sporangia showed a slight flash of color, but no reaction could be observed in the hypha under the microscope. Portions of young mats of *Aspergillus niger* and of *Fusarium* sp. showed brilliant color. Water extracts of these and of colonies of *Erythrobacillus prodigiosus* responded to the reaction. The hypha is probably so small that the amount of reduced glutathione present does not give sufficient color to be discernible. Portions of fresh yeast cake gave a brilliant color, yet no color could be seen in the individual yeast cell.

No color reaction was observed in either *Spirogyra* or *Zygnema*. This may be due to the small amount of the dipeptide in the slender filaments, or it may be that the color was disguised by the large chloroplasts.

(*b*) *Bryophytes.*—Liverworts and ferns were used. Sections through the expanded tops of the male and female gametophytes of both *Marchantia polymorpha* and *Reboulia* sp. showed color in the antheridia and archegonia. The reaction occurred in the antheridial contents in crushed antheridia of *Mnium* and in the region of the spores in the crushed immature capsule.

(*c*) *Pteridophytes.*—Young gametophytes of *Pteris aquelina* showed color in the antheridia and archegonia and a suggestion of color in the young rhizoids. In sections of the rhizome and uncoiling fronds of *Woodsia* sp. a flash of color showed in the meristematic regions and in the vascular bundles. The color, though not the characteristic pink or purplish-red because of the presence of pigment, was evidently, from its behavior, the same type of reaction obtained in other tissues.

(*d*) *Spermatophytes.*—Sections of the seed of *Pinus edulis* showed pronounced color in the embryo, especially at the tip of the hypocotyl, but no color in the megagametophyte. In the Angiosperm, inflorescences, embryos and parts of the flowers, stems and roots responded to the test. In *Brassica oberacea* var. *botrytis* and in *Asparagus* sp. vivid color occurred in those spots where the new floral parts were forming. A paler pink marked the region of the vascular bundles. Sections through the grains of *Zea mays* and *Hordeum* sp. gave no color in the endosperm, but color showed throughout the embryo. The cotyledon was a faint pink, but the plumule, the primary and the secondary radicles were a vivid color.

In the flower the most vivid color occurred in the ovules and throughout the pollen grains. No color was observed elsewhere in sections of the ovaries and anthers except a faint pink in the vascular bundles.

Herbaceous and woody stems, bulbs and tubers were tested. *Lycopersicon esculentum* and *Asparagus* showed color in the region of the phloem. In the latter the color quickly spread throughout the vascular bundle. Twigs of *Prunus Persica* one, two and three years old showed color in the region of the phloem and the cambium. In twigs from staminate and pistillate trees of *Salix*, color occurred in the phloem and cambium, but there was no apparent difference in the intensity of the color in the twigs from the staminate and the pistillate trees. *Gladiolus* bulbs showed faint color throughout the bulb with a more vivid color in the vascular bundles of the leaves and a brighter color in the flower buds. In tubers of *Solanum tuberosum* a well-developed color appeared in the region of the vascular bundles and an intense color in the tip of the bud. A section through the terminal bud of the sprout showed vivid color in the axillary buds with faint color in the phloem.

Longitudinal sections through the apex of primary and adventitious roots showed no color in the root cap. In the embryonic region the color was vivid but became very faint in the region of elongation. In the stele, however, the color extended back into the younger portion of the region of differentiation. The secondary root tips also showed a vivid color. A cross section of the stem of *Lycopersicon esculentum* through an embryonic adventitious root showed color in the vascular bundles of the stem and a much more vivid color in the cone of the embryonic root.

The results obtained by the writer on the distribution of reduced glutathione in plant tissues as indicated by the nitroprusside test show that it occurs throughout the plant kingdom with the exception of the algae. The apparent absence in the latter may be due to the methods of examination and the difficulty of observing color in the thin filaments and highly pigmented material. The substance occurs in those cells which have the capacity of producing new cells—in the primary and secondary meristematic regions of the roots and stems and in the reproductive organs. In addition to these regions it is found in the vascular bundles of those plants which possess vascular systems. In the Angiosperms this substance occurs in the phloem. With the exception of the phloem, the areas which react to the nitroprusside test are rapidly growing areas. There is apparent correlation, therefore, between meristematic tissues which have a high respiratory rate and the areas giving the reaction for reduced glutathione.

Sections of viable corn grains and corn grains which had lost the ability to germinate because of age were tested with nitroprusside. There was no apparent difference between the color obtained in these.

The embryos[2] of corn grains exposed to X-rays[3] at 120 KVP, 4 ma, 18 cm for 94 minutes showed a more intense color throughout than the embryos of un-

[2] Tunnicliffe, *Biochem. Jour.*, 19: 149–8, 1925.

[3] The writer is indebted to Dr. L. J. Stadler for treating the corn and barley grains with X-rays.

treated corn grains. Exposure for 144 minutes increased the intensity of the reaction.

The embryos of soaked barley grains exposed to X-rays for one, two, four and eight minutes showed an increase in the intensity of color in the one-minute exposure with a change in the quality, and decreased intensity of color in the two- and four-minute exposures. The eight-minute exposure showed a very faint pink color.

Attempts were made to find a method for a quantitative determination of reduced glutathione in the higher plant tissues. Tunnicliffe's[2] quantitative method was tried with yeast with results somewhat lower than those obtained by him. The same method was applied to the tissues of higher plants, but the attempts were unsuccessful because of the writer's inability to eliminate the pigments and at the same time retain the substance which reacts with nitroprusside.

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ON THE RECOVERY FOLLOWING LESIONS IN THE CEREBRAL CORTEX

THE recovery of functions following lesions in the cerebral cortex is a common "observation" which has received little or no thorough study. In the literature on this subject that was accumulated during the World War, Hollander has found numerous cases in which the patient, after having lost large quantities of nervous tissue, is said to have "entirely recovered his mental faculties." In many cases the patient demonstrated this complete recovery by sticking out his tongue and walking across the room when told to do so by the attending physician. Obviously a little more testing is necessary before we shall care to place much confidence in such reports.

In the work with experimental lesions the same error has been not quite so obvious. Complete recovery of function has been reported to be found in experimental animals when there have been either no tests at all, or only tests of gross movements such as locomotion. Employing such methods, the major question is, "How great a lesion may be made without producing loss of function?"

If adequate tests are used, if those reactions are tested which the animal finds difficult to make, the interest undergoes a reversal. The question now becomes, "How small a lesion may be made in the cortex and still produce a measurable loss in the animal's reaction capacity?"

In our investigations we are employing cats. The animals are tested before and after the operations on such situations as climbing a vertical screen, to which they hold with three paws while they stretch for food with the fourth; high jumping; climbing a vertical ladder; crawling through small holes; jumping up to catch a rope and hanging there by the forepaws while the food is captured by the head; stretching down from a platform; walking across a narrow bar on which there are obstructions; reaching through a small hole at various angles in both the vertical and horizontal planes; removing a bag from the head, and a few others of a similar caliber.

The potentialities of such an attack may be illustrated by some of the results obtained on three animals.

We removed from the parietal region of No. 32 an amount of tissue which we judged to be about equal to all that forward of the cruciate fissure. Six weeks after the operation our tests could reveal no loss of motor reactions. In the tests made before the operation No. 34 used his left forepaw much more often than he used the right. A lesion about a quarter inch wide and three eighths inch deep was made in the arm area of the right motor cortex. No tissue was removed. Six weeks later the animal was completely right handed. In those situations which demand skilful use of the left forepaw to obtain food the animal goes hungry. All the tissue forward of the cruciate fissure was removed from No. 35. Six weeks later the animal was unable to perform any of the required reactions.

It is interesting to note that at the time of the second test all these cats were "normal" to a casual observer.

There is little evidence here that there is "segmental localization," "equipotentiality of the cortex," "vicarious assumption of function," or that the "brain acts as a whole." Our work, so far, has produced results which indicate that there is organization in the central nervous system. In any organization certain parts have certain functions.

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